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COLORED AFTER IMAGES FROM UNPERCEIVED WEAK CHROMATIC STIMULATION

BY SIDNEY M. NEWHALL AND RAYMOND DODGE

Yale University

THE PROBLEM

The general problem in this investigation is the question of the effects of weak stimuli, especially of such as are too weak to produce either overt response or perception. One might paraphrase the problem as the rôle of sub-threshold stimuli, provided one keep in mind the criteria which determine the meaning of the term threshold. Experimental physiology and psychophysics have concerned themselves largely with the effects of super-threshold stimuli, possibly because they are more directly observable and have a more direct or more apparent bearing on organic behavior. However that may be, evidence is accumulating that alongside overt responses by irritable tissue which seem to follow the all-or-nothing law, there are other less directly observable effects of inadequate stimulation that modify response. Something of this sort appears in the processes of facilitation, summation, and possibly inhibition.

Our specific experimental question was whether one could induce an adaptation of the visual system, resulting in chromatic negative after-images, by chromatic stimuli whose onset and increments were imperceptible. An affirmative answer to the specific question would seem to indicate some change in sensory irritability consequent to faint stimulation

which does not appear to be subject to the all-or-nothing law of neural response and which corresponds to the development and perpetuation of reflex refractoriness by stimuli which are too faint to produce overt reactions.[1] A negative answer, on the contrary, would prejudice the attempt to understand mental inhibition in terms of refractory phase.

The literature of the general problem is indicated in recent papers on inhibition by Dodge, especially in "The problem of inhibition" and in "Theories of inhibition, Part II."[2] The literature of the specific experimental problem is indicated and discussed in papers by Titchener and Pyle[3] and by Ferree and Rand.[4] Both of these papers are concerned with allied problems which are explicitly differentiated in the last-named as follows: "In general two types of method have been used in arousing the color excitation. In one the color was kept below the limen of sensation by adaptation; in the other it was obscured by the action of an unfavorable brightness excitation."[5] We are at present directly concerned only with the first method, with which Tschermak obtained positive results and with which Titchener and Pyle secured definitely negative results.

Method and Results

General Method.—The subjects observed an illuminated field at the center of a relatively large neutral ground. The observation proceeded during adaptation to moderate illumination; it was continuous and monocular, using the dominant eye, and concerned itself chiefly with foveal phenomena. Chroma was introduced into the central field in various amounts and withdrawn from it, the rate of increment or decrement being in some instances slow and uniform and in others relatively rapid. The subjects reported in respect of

[1] R. Dodge, *Zsch. f. allg. Physiol.*, 1910, 12, 54; *Psychol. Rev.*, 1926, 33, 182 f.

[2] *Psychol. Rev.*, 1926, 33, 1–12, 167–187.

[3] E. B. Titchener & W. H. Pyle, On the after-images of subliminally colored stimuli, *Proc. Amer. Phil. Soc.*, 1908, 47, no. 189.

[4] C. E. Ferree & G. Rand, Colored after-image and contrast sensations from stimuli in which no color is sensed, *Psychol. Rev.*, 1912, 19, 195–239.

[5] *Ibid.*, 197.

the apparent color and brightness of the field before and during the introduction of chroma, and during and after its withdrawal. In this way it was possible to correlate the presence or absence of perceived colors and the presence or absence of negative after-images, under various conditions of stimulation and adaptation. The general method, as just outlined, indicates the instrumental requirements.

The essential features of the apparatus may be summarized as follows: (1) An observation-box lighted by two "daylight" bulbs behind milk glass diffusion filters and Aubert diaphragms. (2) A motor-driven color-disc providing for the arithmetical increase of chroma from a neutral zone at the periphery of the disc to a neutral zone at the center, without change in brightness. (3) An optical selective device consisting of a convex lens so oriented between the revolving color-disc and the observing eye that a spot one millimeter in diameter on the disc apparently filled the entire surface of the lens. (4) A driving mechanism consisting of an electric motor operating through a rack and pinion to move the motor-driven disc through the conjugate focus of the lens.

Technical Details.—The subject was seated in a moderately illuminated room with mat black walls, at one end of a table which supported a blackened exposure-box approximately 110 by 60 by 35 cm in size. One end of the box was provided with a circular aperture, *a'* in Fig. 2, and also carried an adjustible head-rest, *h*, which was screened from the direct illumination of the room. Through the aperture the subject could see a neutral gray screen, *s*, within the box. In this screen on the same horizontal axis with *a'* was a second circular aperture, *a''*, which defined the stimulus area. The approximate visual angle subtended by *a'* was 10.7° and by *a''* 1.8°. Thus the subject's total monocular field included a dark outer area and a doughnut-shaped area of gray screen, with the experimental stimulus field in the center.

The aperture in the screen, *a''*, was filled with a convex, ten diopter lens at the center of which was scratched a small cross to serve the observer as a fixation object. The chief purpose of this lens will be described later. Behind the screen was a 25 cm disc, *d*, which could be rotated in a plane parallel to that of the screen. The motor driving this disc was mounted on a small carriage which could be moved along knife-

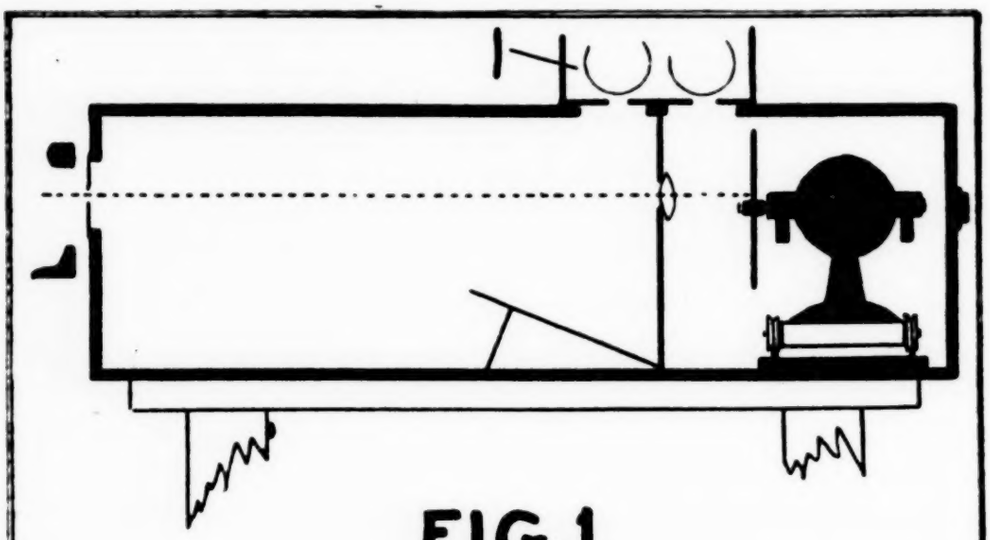

FIG. 1

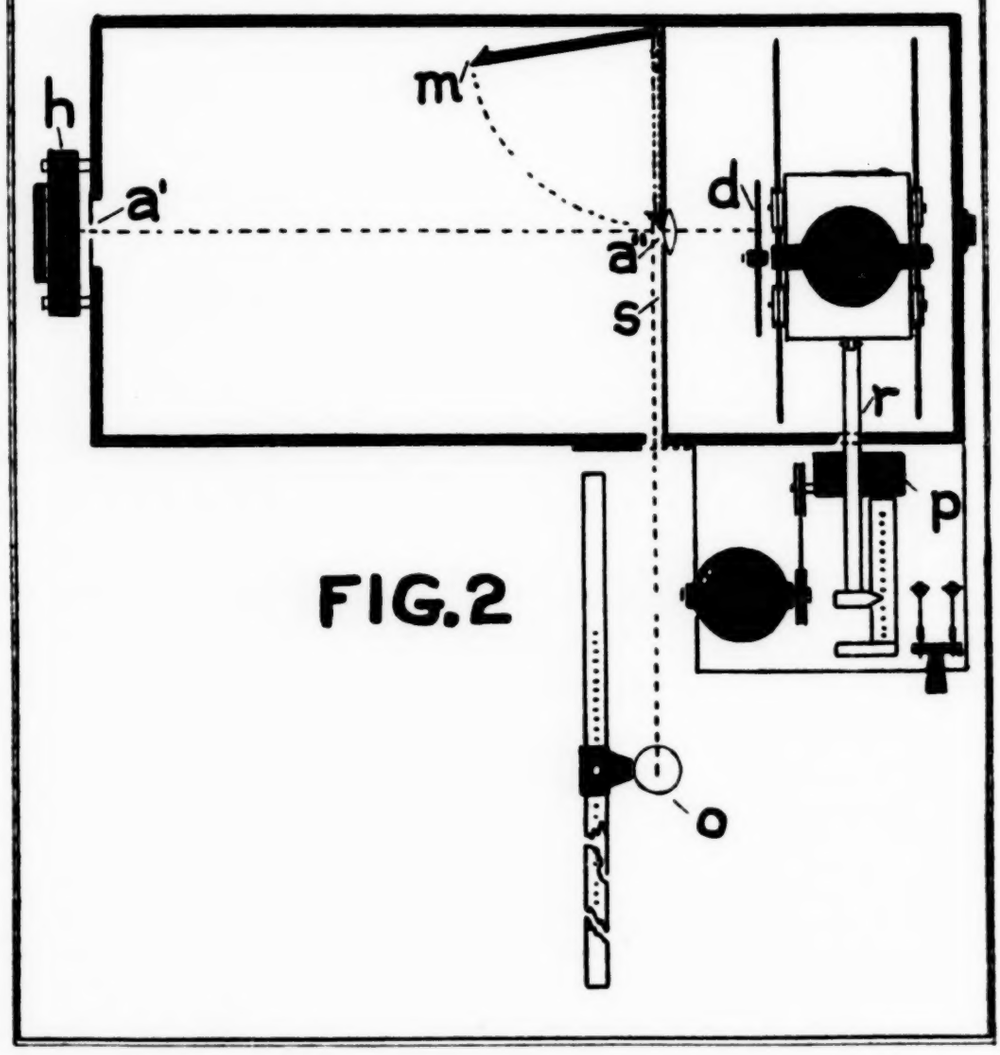

FIG. 2

FIG. 1. Longitudinal section of apparatus in a vertical plane
FIG. 2. Apparatus viewed from above with top of observation box removed

edge-tracks also parallel to the screen. Since the center of the disc was at the height of the aperture, a radial area of the disc would pass behind the lens if the carriage was moved a proper distance along its track. The purpose of the carriage and revolving disc was to present a uniform rate of change of chroma in the stimulus area.

The disc was made of light bristol faced with Munsell grey paper. A leaf-shaped area of Hering colored paper was superposed on the grey. The angular extent of this colored area increased arithmetically from zero or a point at 1 cm from the periphery of the disc to 75° at 1.5 cm from the center. Four discs were prepared with color areas respectively of red, blue-green, yellow and blue. In each case the colored and the grey papers had been photometrically equated in intensity.[6] If the carriage moved uniformly while the disc revolved, the conjugate focus of the lens would describe a spiral on the disc and the chroma of the stimulus at each part of the spiral would be determined by the width of the colored area at that part while the brightness should remain constant.

Two principal rates of change of chroma were used experimentally, (1) relatively rapid, *i.e.*, approximately 75° per second and (2) slow, *i.e.*, 0.6° per second; a ratio of 125 to 1. The rapid rate was manually produced and hence was probably irregular even after extended practice by the experimenter. The slow rate was much more uniform for it was mechanically determined by an electric phonograph motor operating through a rack and pinion, *r* and *p*. These parts are represented in Fig. 2 but not in Fig. 1. Both the driving and disc motors were so wired as to be controlled with a single switch.

The chief purpose of the lens was to present a stimulus field of adequate size and of sensibly uniform saturation at any given moment, from a regularly graduated disc. The color area, as previously stated, increased in angular extent toward the centre of the disc. Consequently, if there had been no lens in the aperture when the rotating disc was observed through it, one side of the central field would have been more saturated than the other. The technical problem was to eliminate this disparity, at least to the extent of achieving apparent uniformity. Relative distances were so arranged that the nodal point of the subject's eye and the surface of the disc were approximately at conjugate foci of the lens when the latter was in an optimal position. Under such circumstances, an area as small as 1 sq mm on the disc behind the center of the lens would, from the subject's point of view, appear to fill the whole area of the lens, *i.e.*, the stimulus field would appear homogeneously filled. Similarly, any concentric band on the disc approximately 1 mm in width which happened to cut the axis of the lens would produce a sensibly uniform field. Probably this field would not always be physically uniform. There might be slight differences in saturation from side to side or from center to periphery of the color area depending on slight displacements of the eye from an optimal position. Our stimulus field 25 mm in diameter was, however, sensibly homogeneous.

The disc and screen were separately illuminated by two 100-watt "daylight" lamps (see top of Fig. 1). Interposed between these sources and the interior of the observation-box were sheets of milk glass and adjustible Aubert diaphragms. The former diffused the illumination and the latter made possible the regulation of the relative intensity of the screen and the stimulus area. To provide for a measure of either intensity or brightness in terms of a recognized unit like the meter-candle, a photometer was constructed which consisted essentially of a movable standard lamp,

[6] The Munsell Laboratory kindly equated the Hering colors with grays and furnished the special gray papers.

o,[7] illuminating a mat surface of oxide of magnesium on plaster of Paris, m, when the latter was swung into position. The brightness of this surface could be equated to that of the screen or the stimulus field by the equality of brightness method. Knowing the candle-power of the source and its distance from the magnesium surface when mean equation occurred permitted description of the illumination of the field in terms of meter-candles, by applying the inverse square law. Brightness could then be expressed in terms of "apparent meter-candles," an apparent meter-candle meaning here the brightness of one meter-candle reflected from the oxide of magnesium surface. In nearly all of the experimentation reported here the brightness of the screen was approximately 4.6 and of the stimulus field 15.2 apparent meter-candles, in other words, the former was ca. 30 per cent. of the latter. These relative brightnesses were used because they seemed to favor observation. In a few otherwise unreported trials during which the screen was unilluminated and appeared black to the subject the control of fixation seemed better while sensitivity to change in the field seemed to be complicated by conspicuous contrast rings. In other unreported trials in which both screen and field were of equated brightness, sensitivity to change of chroma seemed greater but fixation less adequately controlled.

In preliminary experimentation we used the four differently colored discs and ten subjects, some practiced and sophisticated, others unpracticed and naïve. One result was the decision to employ only the Hering R (no. 1) for stimulation in our extended series of observations. The B (no. 13) was rejected because the illumination from the "daylight" sources reflected from our grays seemed yellowish and was subject to some confusion with yellow after-images. The Y (no. 5) was subject to a similar difficulty and both the Y and the BG (no. 10) when mixed with gray and equated in brightness with the other discs, elicited only faint after-images even under most favorable conditions.

Reports during the preliminary experimentation frequently indicated negative after-images without perception of the color stimulus. Concerning the possibility that the experimental conditions might tend to reinforce in some measure the bluish-green after-image through the influence of the Purkinje phenomenon or through brightness more favorable to the appearance of the after-image than to the perception of the stimulating color, as in the study of Ferree and Rand;[8] it seems to us that our procedure adequately controlled the brightness conditions and in some degree the possible influence

[7] The power of this lamp was determined by the Bureau of Standards under our conditions of use.

[8] *Op. cit.*

of the Purkinje phenomenon which, as far as we know, has never been proved to affect after-images in foveal vision.

In view of the results of the preliminary experiments it seemed desirable to confine our subsequent experiments to the red stimulus and relatively practiced observers, with a limited number of variations in the experimental conditions.[9]

Table I shows the relative frequencies of color percepts and after-images when the rate of introduction of the color was 0.6° per second and the rate of reduction was approximately 75° per second. Starting with a gray field the chroma was gradually introduced to 7.5° of the disc when it was reduced to zero without warning. Without pause the chroma was next raised to 15° and then again reduced to zero. This procedure was continued, as indicated in column 1 of the table, until the maximal chroma of 75° had been introduced and withdrawn.

TABLE I

SLOW INTRODUCTION OF CHROMA (HERING RED NO. 1) FOLLOWED BY RAPID WITHDRAWAL, WITH A UNIFORM INCREASE IN THE MAXIMAL CHROMA PRESENTED IN SUCCESSIVE TRIALS

Degrees of Color Presented	Subject D		Subject N	
	Frequency of Color-percepts	Frequency of After-images	Frequency of Color-percepts	Frequency of After-images
0– 7.5	0	0	0	0
0–15	0	2	0	7
0–22.5	3	8	3	10
0–30	3	10	7	10
0–37.5	7	10	10	10
0–45	9	10	10	10
0–52.5	9	10	10	10
0–60	10	10	10	10
0–67.5	10	10	10	10
0–75	10	10	10	10
Total	61	80	70	87
Partial Total*	31	50	10	27

* The partial totals for D are of the first seven figures and for N of the first four.

The subject was requested to fixate the cross at the center of the field (*a''*) as steadily as possible throughout each series

[9] In addition to the writers, this group included Professors E. A. Bott, J. Q. Holsopple and L. T. Spencer. The writers are grateful to them and to a number of other subjects for their time and interest in the investigation.

of 10 trials. The 'ready' signal was the sound of the starting of the motors. The subject was requested to report briefly any changes in brightness or color covering the central area of the field. He was asked to abstract from the contrast or halo effects around the edge of the field. Occasionally the subject reported minute areas or specks of color at various parts of the field or at some particular point, and at times fluctuations in brightness were reported. The specks were sometimes blue and near the center of the field and may have been some variation of the Hubbard effect.[10] Mention of such inconstant and somewhat anomalous phenomena was in time eliminated from the subject's report for, being obviously different in both quality and extension, they were apparently never confused with the stimulus color or the after-image.

In general, the subject reported immediately the changed appearance of the field. Thus, when the stimulus color was observed, it was reported, in any given trial, either during or at the conclusion of the introduction of chroma, while the after-image report occurred during or at the conclusion of withdrawal of chroma. The subjects other than D and N in subsequent parts of the investigation often did not know which was stimulus color and which, if either, after-image.

The table is based on 10 series of 10 trials each, each trial being with a different degree of color stimulation, as indicated in column 1. Reference to the table shows that after-images were more frequently perceived than the stimulating color up to the point where both were perceived with maximal frequency. Both the observed frequencies and the totals indicate that under our conditions the thresholds for after-images were lower than for the stimulus color. Furthermore, these and subsequent data indicate that the color perception of D is less effective than that of N. D's casual experiences and such color equating as he has done lead him to believe that his red perception is subnormal to a slight but measurable degree. It would be interesting to know if a real protanopic subject might fail to perceive the stimulating color and

[10] *Psychol. Bull.*, 1910, 7, 196–199.

nevertheless experience the after-image, with or without progressive adaptation to the stimulus.[11] Our other subjects were questioned in respect of this point and none believed himself protanopic. Our procedure, as will be evident later, was arranged to control such possible defect in considerable degree.

Tables II*a* and II*b*

Slow Introduction of Chroma followed by Rapid Withdrawal, with a Relatively *Low* and Constant Maximal Chroma in Successive Trials

Degrees of Color Presented	Subject D		Subject N	
	Frequency of Color-percepts	Frequency of After-images	Frequency of Color-percepts	Frequency of After-images
0–22.5 throughout	2	8	3	10
	2	9	4	10
	1	8	5	10
	2	9	4	10
	0	10	2	10
	0	8	5	10
	0	10	6	10
	0	10	3	9
	0	10	2	10
	1	10	4	10
Total.....	8	92	38	99
0–22.5 throughout	0	8	5	9
	0	9	2	9
	0	8	5	9
	0	10	3	9
	0	10	5	9
	0	10	3	9
	0	8	3	8
	0	9	6	10
	0	9	4	9
	0	10	6	9
Total.....	0	91	42	90

Tables II*a* and II*b* show the relative frequencies of color percepts and after-images under conditions similar to those for Table I. The chief difference was that for Tables II*a*

[11] Richardson-Robinson reported a blue-yellow color blind individual who experienced red after-images following exposure to a green stimulation which would be definitely superthreshold to normal vision but which for him was imperceptible. *Amer. J. Psychol.*, 1923, 34, 176 f.

and II*b* the same relatively small and uniform amount of color was introduced on each trial. Tables III*a* and III*b* represent conditions essentially identical with those of II*a* and II*b* with the exception that a larger amount of chroma was introduced into the field. Examination of these tables brings out two interesting facts: (1) the preponderance of after-images is related to the magnitude of stimulus, and (2) the results of the various trials are relatively consistent and correspondingly reliable.

Tables III*a* and III*b*

Slow Introduction of Chroma followed by Rapid Withdrawal, with a Relatively *High* and Constant Maximal Chroma in Successive Trials

Degrees of Color Presented	Subject D		Subject N	
	Frequency of Color-percepts	Frequency of After-images	Frequency of Color-percepts	Frequency of After-images
0–37.5 throughout	7	10	10	10
	10	10	10	10
	10	9	10	10
	10	10	10	10
	10	10	10	10
	10	10	10	10
	10	10	9	10
	10	10	9	10
	10	10	10	10
	9	10	10	10
Total.....	96	99	98	100
0–37.5 throughout	9	10	10	10
	9	9	10	10
	7	10	10	10
	10	10	10	10
	10	10	10	10
	9	10	10	10
	9	10	10	10
	9	10	10	10
	9	10	10	10
	7	10	10	10
Total.....	88	99	100	100

Table IV summarizes the results for the five observers following the same procedure as that for Tables II and III. The left half of the table gives the figures for the lesser

stimulus and the right half for the greater. The columns headed "A.I./C.P." give the ratios of frequencies of after-images to color percepts. Thus, when the amount of color introduced was 22.5°, subject D reported negative after-images 22.9 times as often as perception of the stimulating color; but when the stimulus was raised to 37.5° this subject reported after-images only 1.1 times as often as stimulus color. It may be noted that all observers reported after-images more frequently than stimulus color when the lesser value of stimulus was employed. It seems reasonable to suppose that if the stimulus were adjusted with respect for individual limens much more striking results could be obtained.

TABLE IV

SLOW INTRODUCTION OF CHROMA FOLLOWED BY RAPID WITHDRAWAL, WITH CONSTANT CHROMATIC MAXIMA IN SUCCESSIVE TRIALS

Subj.	Cases	0–22.5 Degrees			0–37.5 Degrees		
		Frequency of Color-percepts	Frequency of After-images	A.I./C.P.	Frequency of Color-percepts	Frequency of After-images	A.I./C.P.
D....	200	8	183	22.9	184	198	1.1
N....	200	80	189	2.4	198	200	1.0
B.....	100	83	100	1.2	96	100	1.0
H....	100	36	99	2.8	89	97	1.1
S.....	100	10	88	8.8	89	96	1.1

Whereas for the preceding tables the stimulus color had been introduced at the rate of 0.6° per sec and withdrawn at approximately 75° per sec, for Table V these rates were interchanged, *i.e.*, the chroma was rapidly introduced and slowly withdrawn. Results obtained under this procedure serve as a control on those preceding. Table V is comparable with Table IV, the only change in essential conditions being, as we believe, the interchange of rates mentioned above. Comparison of the tables indicates that the frequency of color percepts is positively related to the rate of introduction of chroma. We thus have some reason to believe it causally related to adaptation rather than to brightness conditions.

It should be noted that the duration and graduation of color stimulation was identical for both tables, but in reverse

direction. It cannot be said, therefore, that the greater frequency of after-images in Table IV is due to greater or longer stimulation than in Table V. The greater frequency of after-images must be related to the rate of withdrawal of the stimulus color. The latter when slow seems to have neutralized or interfered with the development or observation of the after-images. By the time the stimulus had been completely withdrawn the after-image had run its course.

TABLE V

RAPID INTRODUCTION OF CHROMA FOLLOWED BY SLOW WITHDRAWAL, WITH CONSTANT CHROMATIC MAXIMA IN SUCCESSIVE TRIALS

Subj.	Cases	0–22.5 Degrees			0–37.5 Degrees		
		Frequency of Color-percepts	Frequency of After-images	A.I. / C.P.	Frequency of Color-percepts	Frequency of After-images	A.I. / C.P.
D....	100	98	2	0.02	99	6	0.06
N....	100	100	8	0.08	100	21	0.21

Table VI contains limens computed by the constant process which verify in a summary way indications of some of the prior tables. The limens in item 1 in the table were computed from observed frequencies obtained from data upon which Table IV is based. The limens of item 2 were computed from data upon which Table V is based. This "gray limen" is the number of degrees to which the rapidly introduced stimulus color must be slowly reduced before gray is observed as often as stimulus color. The gray limens are seen to be much "lower" than the chromatic limens of item 1. This may be in part due to the neutralizing effect of the after-image mentioned above.

Items 3 and 4 depend upon experimental conditions not yet described. In these experiments the chroma was introduced and withdrawn at the same rapid rate. The duration of color stimulation averaged 30 seconds. Using the method of constant stimuli, seven stimulus values equally spaced from zero to 22.5° were employed. The subjects were requested to report "only change in brightness or color

occurring within about two seconds after the tap (or ready) signal. If 'no change,' report that. After a report attention may be relaxed until the next tap signal, but fixation should be maintained until the 'rest' signal is given." After each trial the subject was given a rest period for recovery from the after-image. It was only feasible to run 15 series, three per session, which is probably inadequate; nevertheless, it seemed best to report these results.

TABLE VI

CONSTANT PROCESS LIMENS

	Subj.	Cases	Rate of Change of Color in Degrees per Second	Limen in Degrees of Color	h
1. Chromatic Limen during slow introduction of chroma.....	D	200	0.6	30.16	.159
	N	200	0.6	28.10	.213
	B	100	0.6	24.28	.132
	H	100	0.6	28.58	.120
	S	100	0.6	30.28	.163
2. Gray Limen during slow withdrawal of chroma.....	D	100	0.6	14.49	.142
	N	100	0.6	15.84	.227
3. Chromatic Limen during rapid introduction of chroma.....	B	75	75.0	20.89	.113
	H	75	75.0	19.81	.133
	S	75	75.0	17.50	.159
4. After-image Limen during rapid withdrawal of chroma.....	B	75	75.0	18.59	.132
	H	75	75.0	19.66	.112
	S	75	75.0	18.16	.122

Items 3 and 4 show that the limens of stimulus color and of negative after-image are not greatly different when the rate of introduction of the stimulus is rapid and equals the rate of withdrawal. Comparison of items 1 and 3 shows, furthermore, a considerable lowering of the limen of stimulus color when the rate of introduction is increased. That the after-image limen is markedly lowered when the rate of withdrawal is increased is shown by comparing item 4 with, say, Tables IV and V. These results tend to confirm those in the previous tables.

It may be recalled that the procedure for collecting the

data of Table I involved a uniform increase in the graduated chroma presented in 10 successive trials, and that there were no rest intervals between the trials. In each trial the chroma was slowly introduced into the field and rapidly withdrawn. Crude limens in terms of medians for the 10 series are given in the first two columns in the body of Table VII. No figures are given for the first two trials of any series because the stimulus color was never perceived at that low value. In trial 3 both subjects reported perception in three out of the ten cases. It was therefore possible to extrapolate medians on the assumption of normality. Even with so few cases, this assumption was probably justified. In the other instances the color was perceived in five or more of the ten cases and the median therefore readily computed. The tabulated figures, as well as the complete distributions corresponding to the stimuli which were always perceived, show a rather regular increase; the threshold appears to rise as the series progresses.

Table VII

Effect on the Chromatic Limen of a Uniform Increase in the Maximal Chroma Presented in Successive Trials

Trial	Median of Perceived Color under Conditions of Table I		Median of Perceived Color under Conditions of Table III*a*		Median of Perceived Color under Conditions of Table III*b*	
	D	N	D	N	D	N
1....	—	—	33.8	26.3	31.9	27.5
2....	—	—	30.8	31	33.8	30.6
3....	24.8†	23.3†	31.9	30.6	35.6	28.7
4....	32.3†	27.5	30	31.9	32.3	30
5....	30	31.3	30.8	31.9	33	30
6....	31.9	33	30	31.6	33.8	28.1
7....	31.3	35.3	28.7	31.9	31.8	29
8....	34.7	35.6	30	31.3	33.8	30
9....	33.8	40.5	32.5	32.9	35.3	27.5
10....	39	43.1	32.9	31.9	33.8	28.1

† This value was extrapolated, on the assumption of normality, from the .3 of the total distribution that was not artificially eliminated by the procedure.

One of the writers was especially interested in this phenomenon and so an attempt at analysis was made. Tables III*a* and III*b*, it may be recalled, exhibit data based on

conditions essentially the same as for Table I, except that a constant value of stimulus was employed. If the progressive rise in the limen based on Table I was due to the progressive increase in stimulus, that fact should be evident in a comparison with limens based on Table III*a* or III*b*. These latter medians of perceived color are shown in the last four columns of Table VII. It will be observed that they do not progress in value as the series advances. Table III*b* really differed from III*a* in that one-minute rest intervals were allowed between the successive trials for recovery from after-images. This variation, however, appears to have had no significant effect on the results as comparison of the last four columns of Table VII indicates.

Several possible explanations of the progressive rise in the limen suggested themselves: there may be an increasing time required for return of the optic mechanism to equilibrium when the chroma is progressively increased, or perhaps the subject realizes that he has an increasing time-period in which to arrive at his judgment, and therefore gradually advances it. There is evidence supporting this last view in the introspections of the subjects and in some of the record sheets.

Summary

1. Negative after-images usually did not result from unperceived color stimulation when the color stimulation was both introduced and withdrawn at the same rapid rate. This result would also be expected for a uniform *slow* rate of change but that has not yet been demonstrated.

2. Negative after-images frequently resulted from unperceived color stimulation when the lack of color perception was a result of an adaptation attained by slowly increasing definitely subliminal stimulation, and the appearance of the after-images was favored by suddenly withdrawing the stimulus color.

3. The stimulus color could be perceived without corresponding negative after-images when the color stimulus was introduced rapidly and withdrawn slowly.

4. The simple limens of color stimuli and of after-images

appear inversely related to the rate of introduction or withdrawal of the stimulus color.

5. Negative after-images from unperceived colors appear to be a fact and also something of an artifact. For when a certain important condition of observation and stimulation, *i.e.*, suddenness of change of stimulation, is more favorable to the appearance of after-images than to the perception of the stimulus color, after-images are the more frequently observed; and *vice versa*. Similarly, when this important condition of observation and stimulation favors the after-images and the stimulus color in approximately the same degree, then the after-images and the color are observed with approximately equal frequency.

6. Our experiments show the possibility of delaying or inhibiting adequate response to color by faint stimuli of the same qualitative order. This effect apparently lies outside the all-or-nothing law. For convenience of reference we call it the negative effect of faint stimulation, without any implication as to its nature or origin.

Additional Problems

In the course of these experiments and their elaboration several related problems suggested themselves. Some of the more promising may be provisionally stated as follows: What are the effects of variation in the brightness and chroma of the peripheral field on the discrimination of slowly changing chroma? What are the optimal form and speed of color increment and decrement—by 'optimum' in relation to our experiments we mean that increment-ratio which permits the greatest degree of stimulation without perception of color—; *i.e.*, the relationship between the curve of optimal subthreshold increments and the curve of increment that just holds color perception constant? Our experiments seemed especially to emphasize the fundamental question as to the nature and locus of that process in the visual system which inhibits without evoking normal reaction. An analogous depression of irritability by superthreshold stimulation is commonly called adaptation, but the name

satisfies neither our curiosity nor the scientific requirements of the present data. Depression of irritability without color perception has several peculiarities. It is a consequence of stimulation that does not appear subject to the all-or-nothing law, since normal response, both sensory and motor, is lacking. It can hardly be regarded as due to exhaustion, since the latter implies previous reaction. Something analogous was found by Dodge in the reflexes;[12] but it seemed impossible to determine the nature of the phenomenon under the conditions of his experiments.

[12] *Op. cit.*

THE INFLUENCE OF PRIMACY

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It has been a common, not to say orthodox, doctrine that other things being equal, the response which is made to a situation by an individual the first time that he encounters it becomes connected with that situation more strongly by that one experience than does any response made to any single later occurrence of the situation by that one experience.

Thus Seashore writes: "In experimental psychology a more specific formulation of laws in terms of force of the association has gained recognition. The chief of these are: (1) *primacy:* other things being equal the association first formed will prevail."[1] Pillsbury writes: " . . . We find the factors that determine the strength of the connection between one element and those that have been associated with it. These have been shown to be the *frequency* with which the two elements have appeared together, the *recency* of their association, . . . and the *primacy* of the association. Professor Calkins has shown that the earlier one element enters into an association with another, the more likely it is to be recalled with that than with any other with which it has been associated at a later period."[2]

We have subjected this doctrine to rather extensive experimental tests, the net result of which is to prove that primacy in and of itself has zero potency.[3] We shall show, for example, that, other things being equal, an individual's second or third response to a situation is as prophetic of, and similar to, his later responses to it as his first response, and that in a series of n recurrences of a situation, the last response is as indicative of, and similar to, responses 2 to $n - 1$ as the first response is, if other things than temporal position are kept equal.

Experiment 1

Fourteen college or graduate students estimated the lengths of 250 strips of paper, 10 of 5-in, 10 of $5\frac{1}{4}$-in, 10 of

[1] C. E. Seashore, *Introduction to psychology*, 1923, 158.

[2] W. B. Pillsbury, *The fundamentals of psychology*, 1922, 285 f.

[3] The investigation reported in this article was made possible by a grant from the Carnegie Corporation.

$5\frac{1}{2}$-in, and so on up to 10 of 11-in. These were presented one at a time, in a random order, being laid on a table in front of the subject, who entered his estimate for each in terms of inches and quarter-inches. No subject knew anything concerning the constitution of the series, save that each length was a multiple of $\frac{1}{4}$-inch. For each length for each person we then have a record of the ten estimates in the order of their occurrence like this for B with the 5-inch length; 3_2, 3_3, 4_1, 3_1, 4_1, 4_2, 4_0, 4_0, 4_1, 4_3 (the first digit of each number denoting inches and the second fourths-of-an-inch). If now the connection producing the response 3_2 has any special potency over behavior due to its being the first, responses of 3_2 should be more frequent in, say, the last seven of the ten than responses of 3_3 or of 4_1 (the second and third). This is on the assumption that there is zero correlation within one length for one individual between the magnitude of his estimates and their place in the series. This assumption is probably correct, but we shall check it later.

The general result of the three hundred and fifty such computations is that the 2,450 responses from the fourth to the tenth displayed 550 like the first, 533 like the second, and 591 like the third. In order to have knowledge of the reliabilities of the 550, 533, and 591, we may express each as the average of five subtotals, the first computed from the facts for 5, 6_1, 7_2, 8_3 and 10, the second computed from the facts for 5_1, 6_2, 7_3, 9 and 10_1, and so on. They then become 110, $106\frac{1}{2}$, and 118, with mean square errors of 6.8, 4.1, and 5.7 respectively. There is thus no evidence of any special potency of the connections productive of the first response.

We can deal with the possible correlation noted above by computing the number of cases where responses I to VII were the same as response X in the series, and comparing the results with the number of cases (550) where responses IV to X were the same as response I. The correlation, if it exists, works against likeness of I to VII to X as much as against likeness of IV to XX to I. The number of cases of the 2,450 responses I to VII which are the same as response X is 522. Using the same procedure to express this with an

estimate of unreliability as was used above, we have Ave. r = 104½, with a mean square error of 4.9. The difference between the 522 and the 550 is less than six-tenths of its mean square error. The absence of any special potency of the connections productive of response I, due to their primacy, is then not easy to explain by a contrary force due to positive correlation of the magnitude of an estimate with its position in the series.

Experiment 2

Strips of white cardboard 4 in. wide and from 12 to 28 in. long were exposed for 2 sec at the rate of one every 5 sec; and the subject was required to make and record an estimate in inches of the length of each. Among the strips were 12 15-in strips, 12 18-in strips, 12 20-in strips, 12 22-in strips, and 12 25-in strips. Each subject's responses to each of these series were recorded in order. The subjects had no knowledge whether their estimates were right or wrong, except that possibly some of them might notice the ratings given by other subjects sitting near them, or get some impression from some subject who tended to whisper to himself the numbers as he wrote them down. They were cautioned about both these matters. We then observe whether the first response has any especial potency over later responses, by observing whether responses II to XI are more like response I than like response XII; or whether responses II to XI are more like response I than responses III to XII are like response II, or than responses I to X are like response XI. Our measure of likeness is the number of the ten which were given the same estimate as No. I (or XII, or II, or XI, as the case may be). The result is emphatic. The number of the ten following I which are the same as I is 937; the number of the ten following II which are the same as II is 1,179; the number of the ten preceding XI which are the same as XI is 1,568; the number of the ten preceding XII which are the same as XII is 1,590. The first impression of a length and the first response to a length give no evidence of greater power in determining the next ten responses than the second impression and response have, or than the eleventh or twelfth have in determining the preceding ten.

If primacy had any potency it was greatly outweighed by other forces which made the first experience less indicative of the intermediate responses than the second, and both the first and the second much less indicative of the intermediate responses than were the eleventh and twelfth.

Experiment 3

Two adults (graduate students) estimated in square inches without fractions each area in a series of 121 surfaces of various sizes and shapes. There were twelve estimations, one a day for twelve days. Neither subject knew whether any estimate was right or wrong, nor, except for her memories, whether any estimate was the same as any one she had previously made for that area.

We have then 242 records like that shown below for one area.

Occurrences	1	2	3	4	5	6	7	8	9	10	11	12
Estimates	27	30	38	34	31	33	32	34	36	34	35	35

We measure the extent to which the estimates for occurrences II to XI inclusive were like the estimate for occurrence I, and also the extent to which they were like estimate XII. We also measure the likeness of estimates III to X inclusive to estimate II, and to estimate XI. Likeness is measured by the number of estimates identical with I or XII or II or XI, as the case may be. The results are as shown below.

	Subj. R	Subj. W
Occurrences II to XI	1,210	1,210
Number like I	138	81
Number like XII	212	254
Occurrences III to X	968	968
Number like II	129	109
Number like XI	203	206

Here again there is no evidence of any superior potency of the first experience. If there is any such potency, it is far outweighed by factors which cause a reduction in the variability of the responses to the same surface.

Experiment 4

The subject, being given 6 sheets of paper 8½ in by 11 in, was told to close his eyes and draw a line 2 in. long, then below it a line 4 in. long, then below that a line 6 in. long, drawing four such sets of three on each sheet. Each line was to be drawn with one fairly quick swing of the hand. This was illustrated for him. He then closed his eyes and drew the 24 lines. The length of each was then measured to the nearest tenth of an inch. The number out of the last twenty 2-in lines which were of the same measured length as the first 2-in line drawn was recorded. The number out of the last twenty 2-in lines which were of the same measured length as the second 2-in line drawn was recorded; and similarly for the number of the same length as the third 2-in line drawn and the fourth.

If the connection producing the first response to the situation "Draw a two-inch line," has an advantage over the connection producing the second or third or fourth, by reason of being the first, and if other things are equal, the number of the responses V to XXIV like response I should be greater than the number like response IV.

Similar computations were made in the case of the 4-in lines and 6-in lines. Also similar computations were made for all three lengths, using the number of responses V to XXIV where the measured length diverged from that of response I or II or III or IV by 0.1 inch. The facts appear in Table I for fourteen adult subjects. If there were no relation between the serial order of a subject's drawings of a length and their magnitudes, this table could be taken at its face value, and would show a decided inferiority of the connection productive of the first response. There is, however, such a relation in many of the individuals, as is shown by the differences between the median length of the first 9 lines drawn and the median length of the last nine; and allowance for it must be made. Allowance can be made by comparing the potency of I, II, III and IV on V to XXIV with the potency of XXI, XXII, XXIII and XXIV on I to XX.

We find that the influence of connections XXI to XXIV on connections I to XX is in general equal to the influence of connections I to IV on connections V to XXIV. The number of zero divergences is 192 for the former and 185 for the latter. Corresponding numbers for divergences of one are 289 and

TABLE I

THE INFLUENCE OF PRIMACY IN THE CASE OF DRAWING LINES

Subj.	Length of First 4				No. Lines in Trials V–XXIV Equal to				No. Lines in Trials V–XXIV Differing by 0.1 inch from				Median Length of	
	I	II	III	IV	I	II	III	IV	I	II	III	IV	First 9 Lines	Last 9 Lines
In drawing 2-inch lines														
Bre.	13	20	24	23	1	2	3	1	3	3	3	3	24	20
Bu.	11	11	10	10	0	0	0	0	0	0	0	0	13	22
D.	11	14	15	11	6	4	2	6	5	3	5	4	13	12
F.	10	8	11	13	3	1	5	3	10	5	4	2	11	11
H.	9	6	5	5	0	0	0	0	5	0	0	0	9	13
K.	11	9	8	11	2	1	0	2	1	0	1	1	11	17
L.	6	18	18	6	9	0	0	9	4	0	0	4	6	6
M.	16	19	17	20	1	2	3	5	3	7	3	4	17	20
R.	10	10	9	5	2	2	1	0	1	1	2	0	10	16
Sn.	18	21	25	28	0	0	2	1	1	1	3	0	25	25
Sw.	6	9	10	8	0	0	3	3	1	6	6	0	9	13
V.	12	21	18	16	2	1	2	4	0	0	4	7	16	17
Y.	11	12	19	19	0	0	3	3	0	1	8	8	19	18
Totals.					26	13	24	37	34	27	39	33		
In drawing 4-inch lines														
Bre.	39	41	45	49	1	0	1	0	2	5	1	2	45	39
Bu.	23	18	21	21	0	0	0	0	4	0	1	1	23	36
D.	21	26	30	30	0	2	3	3	0	1	5	5	29	30
F.	19	22	24	21	0	1	1	0	0	1	1	1	24	27
H.	14	15	16	17	0	0	0	0	0	0	0	0	21	26
K.	30	20	23	22	1	0	0	0	5	0	1	0	26	33
L.	18	20	16	14	3	2	3	2	5	2	4	4	18	16
M.	38	36	29	37	1	1	0	0	0	3	3	2	36	36
R.	20	25	32	21	0	1	1	0	2	1	4	1	22	30
Sn.	28	35	44	49	0	0	0	0	0	1	2	1	49	46
Sw.	11	15	17	13	0	1	3	0	0	0	2	0	17	22
V.	27	36	29	31	1	0	3	1	2	1	4	6	31	30
Y.	25	23	26	41	0	0	0	4	0	0	0	5	37	37
Totals.					7	8	15	10	20	15	28	28		

TABLE I.—*Continued*

Subj.	Length of First 4				No. Lines in Trials V–XXIV Equal to				No. Lines in Trials V–XXIV Differing by 0.1 inch from				Median Length of	
	I	II	III	IV	I	II	III	IV	I	II	III	IV	First 9 Lines	Last 9 Lines
	In drawing 6-inch lines													
Bre	41	44	63	58	1	0	0	0	1	2	0	1	56	47
Bu	34	34	31	32	2	2	1	0	2	2	0	3	33	51
D	41	38	48	30	2	2	1	0	3	2	2	0	38	42
F	36	45	43	41	0	0	3	1	2	0	2	6	43	39
H	19	20	20	23	0	0	0	0	0	0	0	1	24	32
K	36	34	31	31	0	0	1	1	3	3	1	1	33	48
L	27	26	22	25	4	0	0	5	0	9	5	1	25	25
M	58	44	35	49	1	0	2	0	1	1	0	1	46	52
R	36	35	40	38	1	1	2	1	1	1	0	0	36	46
Sn	49	42	54	65	0	0	0	2	0	0	0	4	66	61
Sw	16	18	21	21	0	0	1	1	0	0	1	1	21	28
V	45	41	35	38	0	0	2	0	1	0	3	4	38	35
Y	26	31	40	47	0	0	2	3	0	0	1	1	47	44
Totals					11	5	15	14	14	20	15	24		
Sum of 3 totals					44	26	54	61	68	62	82	85		

297. Using divergences of 0 or 1, we have 481 and 482. The influence of connection XXIV on connections I to XX is practically as great as the influence of connection I on connections V to XXIV. The zero divergences are 44 for I and 42 for XXIV; the divergence of one-tenth inch are 68 for I and 68 also for XXIV.

Experiment 4 then in no wise supports the doctrine that the first connection made with a situation is more important than any other in determining future connections.

EXPERIMENT 4*a*

Experiment 4*a* was the same as Experiment 4 except that only two lengths, 3-in and 5-in, were drawn, and only 12 lines of each length. There were 124 subjects. The lines were all measured to 0.1 inch. We count the number among lines II to XI which received the same measure as line I, and the

number which received the same measure as line XII. The results are: for 3-in lines; 104 like line I, 133 like line XII; for 5-in lines, 101 like line I, 110 like line XII. We also count those whose measures were only 0.1-in each up or down from line I, and similarly for line XII. The results are: for 3-in lines, 210 within 0.1-in of line I, 225 within 0.1-in of line XII; for 5-in lines, 159 within 0.1-in of line I, 174 within 0.1-in of line XII.

Experiment 5

In Experiment 5 the external situations presented were the sound of a (as in late), ee (as in week), ou (house), o (home), aw (saw), k (kind), f (fence) and s (so). The response was, in each case, to represent the sound by a letter or letters. The sounds appeared in three-syllable nonsense words [4] which the subjects spelled. For each sound for each of 105 subjects, we have a record of the first twelve spellings in order.

We compare the occurrences, in responses III to X, of the spellings which were given the first time that a sound occurred, the second time, the eleventh time and the twelfth time respectively. The totals are 3,398, 3,485, 3,626 and 3,627. The first response thus has no advantage over the second or twelfth or thirteenth; but, on the contrary, is less potent in determining what the eight responses from the third to the tenth are to be. The lack of special potency of the first experience is seen even more closely when we eliminate the many cases where the second (or eleventh, or twelfth) spelling, as the case may be, was the same as the first. The frequencies in III to X of spellings I and II, when these are different, are 902 and 991. Those of I and XI, when these are different, are 1,016 and 1,237. Those of I and XII, when these are different, are 898 and 1,136.

Experiment 6

Six college students added letters to make words out of 960 combinations like ca, pl, ti, de, fe, ag, *etc.*, doing 240 a day. In each day's 240, certain combinations occurred twice. For these combinations we have records of the eight completions such as are shown below:

[4] *E.g.*, 1. kace eed′ aub, 2. weece′ o leet, 3. kawl awt eez′, 4. roce′ ane our, *etc.*

ac count	an imal	ap e
tor	nounce	pear
count	other	pear
cept	on	ple
count	t	ple
count	imal	pear
count	t	e
curate	imal	point

We measure the likeness of completions II to VII, inclusive, to completion I and to completion VIII, by counting those which are the same as I and VIII respectively. Since there is a tendency, as the situation recurs, increasingly to favor the shorter completions, we make the comparison separately for cases where completions I and VIII were of the same length, for cases where I was a one-letter and VIII was a two-letter completion, for cases where I was a two-letter and VIII was a one-letter completion, for cases where I was a one-letter and VIII was a three-letter completion; and so on for cases of 3 and 1, 1 and 4, 4 and 1, 1 and 5, 5 and 1, 2 and 3, 3 and 2, 2 and 4, 4 and 2, *etc.*, *etc.*

The results are as follows: In 316 cases where the completions I and VIII were of the same length, 225 were cases where I and VIII were the same completion; in the remaining 91 cases 103 of the 546 completions were the same as completion I, 186 were the same as completion VIII, 257 being like neither. The cases where the completions I and VIII were of different length are treated by finding the average number (out of 6 as a possible maximum) which were like I and the average number which were like VIII. The results are shown in Table II.

In each pair of these averages the two members are given equal weight in computing the result entered in the last two columns. In 16 of the 17 averages of averages, the responses II to VII are more like VIII than like I. If we use the five averages of averages where the sum of the two n's is 20 or more, giving equal weight to each of the five, we have 1.0 for the average number out of the 6 like I, and 2.2 for the

average number out of the 6 like VIII. Using the five averages of averages where the sum of the two *n*'s is 10 to 19 in the same way, we have 1.3 and 1.9. Using the remaining seven in the same way, we have 0.8 and 1.9. Completions II to VII are then unquestionably much more like Completion VIII than like Completion I. Experiment 6 thus lends no support to the doctrine that primacy is effective.

TABLE II

THE INFLUENCE OF PRIMACY IN COMPLETING WORDS

Number of Letters		*n*	Ave. No. in II–VII Like		Number of Letters		*n*	Ave. No. in II–VII Like		Ave. of the Two Aves. Like	
I	VIII		I	VIII	I	VIII		I	VIII	I	VIII
1	2	14	1.0	2.7	2	1	22	1.0	2.0	1.0	2.3
1	3	6	3.0	1.3	3	1	11	0.6	3.4	1.8	2.3
1	4	7	3.0	1.7	4	1	11	1.2	1.9	2.1	1.8
1	5	2	2.5	0.5	5	1	7	0.4	3.1	1.4	1.8
1	6	3	1.0	0.3	6	1	5	1.0	2.4	1.0	1.4
1	7	1	2.0	2.0	7	1	2	0.5	3.0	1.3	2.5
2	3	16	0.7	2.3	3	2	27	1.0	1.9	0.9	2.1
2	4	10	1.3	1.4	4	2	11	1.3	1.7	1.3	1.6
2	5	6	0.5	2.8	5	2	4	0.0	1.3	0.3	2.0
2	8	1	0.0	0.0	—	—	—	—	—	—	—
3	4	16	1.1	2.0	4	3	8	0.3	2.6	0.7	2.3
3	5	7	0.7	1.9	5	3	11	1.7	1.7	1.2	1.8
3	6	2	0.5	1.5	6	3	8	1.3	2.1	0.9	1.8
4	5	15	0.8	2.5	5	4	10	1.3	2.8	1.1	2.7
4	6	4	0.5	3.8	6	4	5	1.6	1.8	1.1	2.8
4	8	1	0.0	2.0	8	4	1	0.0	0.0	0.0	1.0
5	6	5	1.2	1.4	6	5	3	0.0	4.3	0.6	2.9
6	2	3	0.0	1.0	—	—	—	—	—	—	—
6	8	1	0.0	1.0	8	6	1	0.0	1.0	0.0	1.0
7	2	1	0.0	2.0	—	—					
7	4	3	1.7	1.7	—	—					
7	5	2	2.0	3.5	—	—					
8	3	1	0.0	4.0	—	—					
11	4	1	0.0	0.0	—	—					
11	9	1	2.0	4.0	—	—					

In general, the first connection shows no greater potency over the *n* connections immediately following it than the second connection does over the *n* connections immediately following it.

In general, also, the first connection shows no greater potency over those following it than the *n*th experience shows over those *preceding* it. On the contrary, in four of the six

experiments there is much greater resemblance to what comes after than to what comes before. This excess is presumably due in general to a tendency for the variability of responses to the same situation to become reduced.

We have not been able to find any clear evidence in the literature of association in support of the doctrine that the first connection made with a situation has, by being first, any advantage. The experiments of Calkins referred to by Pillsbury do not, as we interpret them, necessarily show that "the earlier one element enters into association with another, the more likely it is to be recalled with that than with any other with which it has been associated at a later period"; but rather that being first in a series of twelve paired-association tasks has an advantage over being in the middle of such a series. It seems probable that the advantage is not due to primacy in and of itself, but to the factors which make both the beginning and the end of a series better remembered than the middle.

It seems probable that the facts, to explain which the poctrine of primacy was fabricated, are better explained by two corollaries of the general laws of learning. The first is that, other things being equal, the stronger a connection is, the oftener, and so the earlier, it will show itself. Being first does not make a connection stronger, but being strong makes a connection likely to be first. Let $S_1 \rightarrow R_1$, $S_1 \rightarrow R_2$, $S_1 \rightarrow R_3$, $S_1 \rightarrow R_4$ have strengths of 5, 2, 2 and 1, respectively. Then when S_1 occurs R_1 will be the first response five times as often as R_4 will, and also it will later occur five times as often.

The second is that when responses are connected with situations to which there are no pre-existing connections of more than infinitesimal strength (as in some cases of learning the names of strangers, the English equivalents of foreign words, and the like), the first experience raises the frequency from approximately 0 to 1, the second experience of the same connection raises it from 1 to 2, and so on. The *relative* strength of the connection with its competing infinitesimals is raised to 1–to–0 by the first experience, and only to 2–to–0 by the second experience. This is an effect of frequency caused

by position and is allied to the facts of diminishing returns and overlearning. It is different from the alleged primacy effect. If, in such a case, we have two different responses, one in first position and the other in second position of occurrence, each will be raised to a relative strength of 1–to–0 compared with the infinitesimals; and the one in first place will be at no advantage over the one in second place.

These two simple facts, plus certain influences of novelty on attention, seem likely to be adequate to account for all that is true in the popular doctrine that first impressions are strongest.

The advantage for attention or memory of the first event of a series over later events in the series is, of course, a matter with which primacy, as defined here, has nothing to do.

We conclude, therefore, that the law of use or frequency does not need to be supplemented by any potency of primacy and that the general theory of connection-forming may be relieved from the incubus of the special theory that the response made by an individual to a situation the first time that he encounters it becomes, by virtue of that one experience, more strongly connected with that situation—other things being equal—than does any response made to any single later occurrence of the situation by virtue of a like single experience.

THE RELATION OF RETENTION TO THE DISTRIBUTION OF RELEARNING

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Introduction

Many a psychologist bent upon some form of application has inferred that since the course of associative retention, as formulated by Ebbinghaus and corroborated by various later investigators, exhibits the characteristic of negative acceleration, the distribution of relearning should be so adapted as to conform to this formula in order to procure its maximal efficiency in retention. Such an inference, expressed with various degrees of confidence, often appears, *e.g.*, in educational psychology as well as in the psychology of advertising. In the former, Starch (6) writes: "Since the rate of forgetting is very rapid at first and more gradual later on, it probably would be highly advantageous to have relearning of a given material come very frequently at first and more rarely later on." In the latter, the same inference is made by Hollingworth (3), Kitson (4), Adams (1), Starch (7), and Poffenberger (5). Nevertheless, there has never been offered adequate experimental support to the inference.

The following experiments were designed primarily to test the validity of the inference, or, to put it in more specific terms, to determine the relation of the course of retention to the distribution of relearnings.

Our Experiment I was conducted with the purpose of discovering whether the negatively accelerated characteristic of Ebbinghaus's curve is applicable to retention (as implied, *e.g.*, in advertising materials) under the presentation of paired associates and measured by the method of *aided-recall* (2). Experiment II was undertaken to compare the relative efficiency of three various modes of distribution of repetitions:

one in accordance with negative acceleration, another with positive, and a third without acceleration. Experiment III varied from Experiment II in that its distribution started without any degree of original learning at all. Consequently it should be termed, more appropriately, an experiment on the distribution of learning. It was undertaken in view of the fact that, in a great many cases, the distribution of "appeals," as in advertising, is, in strictness, a distribution of learning rather than of relearning. Its object was also to compare the relative efficiency of the three different modes of distribution already described.

Now if Ebbinghaus's formulation is applicable to such problems as advertising, then the result of Experiment I should turn out to be a negatively accelerated curve, and if the inference concerning the efficiency of distribution of relearning is valid, then the result of Experiments II and III should appear strongly to favor that mode of distribution which is represented in the curve showing negative acceleration.

Experiment I: Retention for Paired Associates

The material used in this experiment consisted of ten paired associates. The first member of each pair was a common English noun representing the name of a certain commodity; while the second member, which corresponds to its trade-name, was composed of two syllables such as are often found in romanized Chinese trade-names. The ten pairs are as follows:

Pencil	Muilap
Watch	Tailan
Shirt	Bensok
Fruit	Haisen
Bread	Soipen
Books	Hontek
Shoes	Yunkon
Candy	Lenkwi
Hats	Kinhap
Soap	Kwaton

The apparatus employed was a modified Jastrow drop-exposure.

Since every moderately loud noise may disturb the subjects during experimentation, the stock Jastrow apparatus proved unsatisfactory to our purpose. To remedy this defect a pulley and cord at the top of the apparatus allowed the serial exposures to be made smoothly one after another without producing the slightest disturbing sound. The exposure card with its wooden frame was counter-balanced by a weight which was hung at the end of the cord along the back of the apparatus and which was lifted by the right hand of the experimenter. The thumb nail of his left hand was put at the proper place along the lower part of the apparatus marked by shallow grooves with prescribed equal distances between, in order to prevent the wooden frame from coming down too far. A stop-watch was attached to the back of the apparatus near the eye-level of the experimenter so that he could easily read it off and raise the weight at the end of each prescribed interval of time. The aperture (2 x 9 in) was placed approximately at the eye-level of the subject. The ten pairs of words were written with India ink on a piece of cardboard which was then fastened to the wooden frame by means of thumb-tacks. In writing the characters, enough space was left around them to make them clearly seen through the aperture from a considerable distance.

One hundred and two college students served as subjects. They were divided into seven groups, each of which was composed of from 12 to 18 individuals. They were all Chinese except one, a Korean. Most of them had had a course in elementary psychology; but none of them had had any previous experience in the laboratory with such material as ours. All of them were sufficiently far advanced in English to remove the difficulty of learning common English nouns and syllables.

All the conditions for the various groups were identical excepting the rest-interval; *i.e.*, the time between the first and second aided-recall, which varied in length from one to fourteen days. The interval of rest for group i was 14 days; for group ii, 10 days; for group iii, 7 days; for group iv, 5 days; for group v, 3 days; for group vi, 2 days: and for group vii, only one day. The laboratory periods for the various groups were previously arranged in such a way that their relative lengths of interval prior to the test series (*i.e.*, the second aided-recall which took place simultaneously for all groups) were the same as the days indicated in the above program. The schedule is shown in the following table:

TABLE I

TIME SCHEDULE OF THE LABORATORY PERIODS OF DIFFERENT GROUPS

Days	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Group No. at date of learning...	i				ii			iii		iv		v	vi	vii	Test (simultaneous second aided-recall)
Rest-interval (in days)..	14				10			7		5		3	2	1	

The instructions were written in Chinese on the blackboard. Every member of each group was asked to read it carefully before the experiment was begun. In addition, the experimenter orally explained the instructions in Chinese so that every one was clear as to the procedure. The following is a literal translation of the instructions:

> This is an experiment in the psychology of learning. Its purpose is to find out how much one can learn within a certain limit of time. Ten pairs of associated words will be exposed to you one after another through the aperture of the apparatus and you are required to learn attentively each pair of them in its proper association. The first member of each pair is always an English noun, while the second member is composed of two syllables. Now your business is to associate the two members of each pair in your memory. After eight trials, you will be asked to supply the right associate for each of the ten English nouns.

The ten pairs of words were presented successively for eight trials, each pair for ten seconds. Accordingly 100 sec were required for each trial. A recess-period of 20 sec was inserted between the successive trials to prevent fatigue and to facilitate memorization. Immediately after the eight trials each subject was given a list of the first members of the ten pairs arranged in a new order, and was asked to write down after them, from memory, their right associates. No statement was made to any subject concerning the requirement of a subsequent trial, *i.e.*, a second aided-recall. One day after the last group (Group vii) had repeated the ten pairs for eight trials each, followed immediately by its first aided-recall, the experimenter sent a note to every subject of each group, requesting him to appear in the laboratory for another experiment (which in reality was but a test of retention or the *simultaneous second aided-recall*). As this took place at

the usual prescribed laboratory period (2 to 3, afternoon) the time elapsed for different groups thus varied from one to fourteen days.

According to our method of scoring, each of the ten associates, if rightly recalled, is given ten per cent., while in case only one of the two syllables in any associate is rightly recalled, then only five per cent. is given. Mis-spellings or wrong associations receive no mark at all. The same criteria apply both to the first and to the second aided-recalls.

The amount of retention is expressed by the ratio between the score of the second and the first aided-recalls. The following is the formula we used:

$$\text{Amount of retention} = \frac{\text{Score of the second aided-recall}}{\text{Score of the first aided-recall}} \times 100.$$

The results thus obtained are given in Table II and graphically represented in Fig. 1.

TABLE II

AMOUNTS OF RETENTION FOR VARIOUS REST-INTERVALS

Rest-Interval (days)	1	2	3	5	7	10	14
Amount retained (per cents.)....	65.6	57.5	54.6	47.2	43.7	36	29

From the above table we may conclude that the curve of retention for such materials, as determined by the method of paired associates and measured by the method of aided-recall, exhibits the characteristic of negative acceleration. The most rapid rate of forgetting occurs during the first two days.

EXPERIMENT II: DISTRIBUTION OF RELEARNING

The present experiment was identical in many respects with the previous, except for the distribution of relearnings.

Seventy-two members of the class in elementary psychology served as subjects. Most of them were sophomore students. They were divided at random into three groups. Group *A* was composed of 21; group *B*, 28; and group *C*, 23 individuals.

Each group learned and relearned the material in nine trials, which were distributed within the same eleven days. Three of the nine trials were presented successively to all the groups during the first day as their original learning, while the other six trials of relearning were being distributed differently for different groups, according to three hypothetical curves of retention. Group *A*, with its initial frequency of distribution and its gradual increase in the length of interval between subsequent trials, was supposed to conform to the negatively accelerated curve. Group *B*, with its distribution just in reverse to group *A*, was supposed to conform to the positively accelerated curve. Group *C*, with its constant temporal schedule all the way through, was supposed to conform to the linear type of distribution.

The following is the time schedule for the three various modes of distribution:

TABLE III

Days	1	2	3	4	5	6	7	8	9	10	11	14	18
Group *A*.....	1st–3d	5th	6th	7th			8th				9th*	1st aided-recall	2nd aided-recall
	4th												
Group *B*.....	1st–3d				5th			6th	7th	8th	9th		
	4th												
Group *C*.....	1st–3d		5th		6th		7th		8th		9th		
	4th												

* Numericals after each group from 1st to 3d represent the first three trials arbitrarily defined as original learning, while the rest (*i.e.*, from 4th to 9th) indicate the trials of relearning distributed at different days.

Three days after all the groups had completed their respective ninth trials, which occurred at the same day, every subject was called in to write down what he could remember of the ten double-syllables in their right association with the ten English nouns which were arranged in a different order. This constituted the first aided-recall. Four days later, *i.e.*, one week after the last trial of relearning, the subjects were called in again for a second aided-recall.

From the above description, it is clear that the three groups were similar concerning (1) the condition of learning, (2) the time-interval between the first trial of learning and the last trial of relearning, and (3) the time interval between the last trial of relearning and the first or the second aided-recall. The only difference between the groups lies in the various modes of distribution of relearning.

The data thus obtained are presented in Table IV.

TABLE IV

RELATIVE EFFICIENCY OF THREE VARIOUS MODES OF DISTRIBUTION OF RELEARNING

Group	Distrib'n of Relearning Time	Retention After 3 Days	Retention After 7 Days
		(per cents.)	(per cents.)
A.....	Negatively Accelerated Distrib'n	61.1	45.5
B.....	Positively Accelerated Distrib'n	52.3	41.0
C.....	Linear Distrib'n	47.6	35.5

The above data justify the statement that, among the three various modes of distribution of relearning, the negatively accelerated ranks highest; the positively accelerated, next; while the linear stands invariably lowest in efficiency as measured by relative amount of retention both after three and after seven days.

EXPERIMENT III: DISTRIBUTION OF LEARNING

In this experiment everything was identical with Experiment II except (1) the degree of original learning; (2) the length of interval between the first and the last trials of learning; (3) the number of subjects, and (4) the series of material.

The series of material used was different from that of Experiment II because some of the subjects served also in that experiment.

Fifty-five students from the class in elementary psychology were divided at random into three groups. Group *A*, nineteen individuals, was to follow a schedule of distribution of learning according to the curve of negative accelera-

tion; Group *B*, nineteen individuals also, was to follow a schedule in accordance with the positively accelerated curve; while Group *C*, seventeen individuals, was to proceed in accordance with the linear type of distribution. The temporal schedule for Group *A* was once-every-day for the first successive four days, then once every-other-day for the next four days, then once in three-days, and then once in four-days. For Group *B*, the reverse schedule was followed. For Group *C*, a constant temporal schedule was adopted; namely,

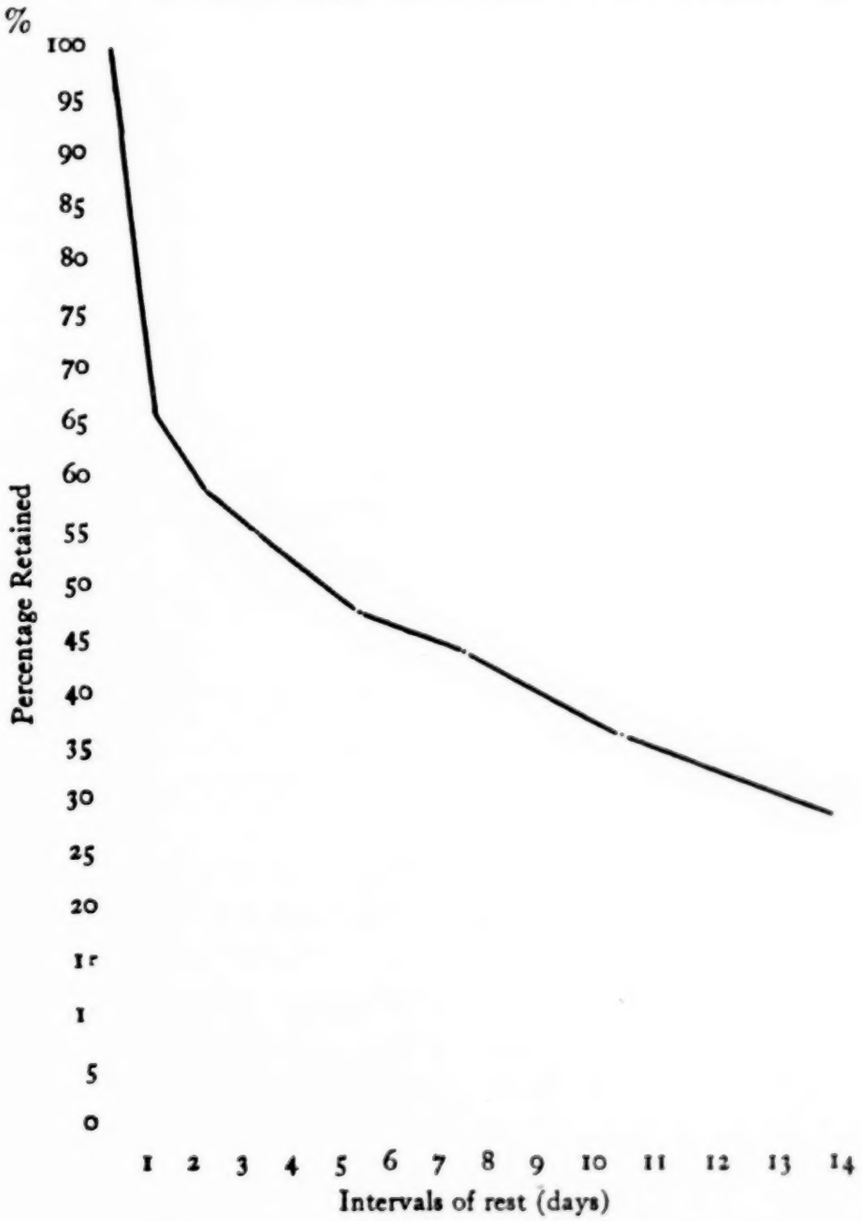

FIG. 1. Curve of Retention for Paired Associates

always once every-other-day. Each group had altogether eight trials, distributed differently within the same fifteen days. The first aided-recall was given three days and the second aided-recall seventeen days after each group had completed its respective eighth trial.

The data thus obtained are presented in Table V.

TABLE V

RELATIVE EFFICIENCY OF THREE VARIOUS MODES OF DISTRIBUTION OF LEARNING

Group	Distrib'n of Learning Time	Retention After 3 Days	Retention After 17 Days
		(per cents.)	(per cents.)
A.....	Negatively Accelerated Distrib'n	74.2	54.4
B.....	Positively Accelerated Distrib'n	63.7	48.3
C.....	Linear Distrib'n	56.4	40.3

From these data we may infer, as in Experiment II, that among the three various modes of distribution of learning, the negatively accelerated distribution ranks highest; the positively accelerated distribution next; and the linear lowest in efficiency, as measured by their relative amount of retention either after three or seventeen days.

CONCLUSIONS

1. The curve of retention for our materials, as studied by the method of paired associates and measured by the method of aided-recall, exhibits the characteristic of negative acceleration.

2. The results on the distribution of relearning of such materials, after a partial degree of original learning, stand strongly in favor of that mode of distribution with initial frequency followed by gradual increase in the length of time-interval between subsequent trials. The results upon the distribution of learning show practically the same thing.

3. So far as the negatively accelerated curve is concerned, our experimental data seem to show that its relation to the distribution of relearning is positive. As to other types of

retention (8), we shall leave the matter open for a later investigation.

4. The inference made in advertising practice that, since the curve of retention exhibits the characteristic of negative acceleration, the distribution of relearning ought to be so adapted as to conform to that type of curve in order to procure its maximal efficiency seems hereby proved to be valid, as both our material and our method of measurement and presentation resemble very closely the practical situations there involved.

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INTELLIGENCE AND SUSCEPTIBILITY TO THE MÜLLER–LYER ILLUSION

BY H. R. CROSLAND, H. R. TAYLOR, AND S. J. NEWSOM

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This study[1] is mainly concerned with the relation of intelligence to the errors of length in the Müller-Lyer illusion. Hollingworth (12) has contended on the basis of experimental results that the more 'intelligent' persons, *i.e.*, those with firm and steady habits of attention, are the more susceptible to the illusion. Bates (1), hoping to confirm Benussi (6) in finding that the analytical attitude reduces the amount of the error of perception or judgment, failed to reduce the perception to a simple process with the result that the illusoriness of the figure persisted.

Any attempt to solve the question raises many difficulties. In the first place, the investigator must decide upon a suitable example of the Müller-Lyer figure itself, since not less than twenty variant forms of it (25, 38) are present in the literature. In the second place, the methods of presenting the figure are almost as numerous as the variations in the figure itself. One may present the lines vertically or horizontally, closely adjacent or widely separated, one over the other or both in a straight line, parallel or non-parallel, to one eye or both and with or without adjustive manipulation by the subject. Again, having found a suitable form of the figure, how and to what degree will varied methods of presentation affect the illusion? If intelligence has any relationship to susceptibility to magnitude of the illusion what effect on this relationship will varied methods of presentation exhibit? Finally, an investigation of this sort cannot escape the possible influence of antecedent practice and acquaintance with the figure.

To meet these difficulties we have used a constant form of the Müller-Lyer figure, a constant place of presentation and orientation of the observer, unvaried conditions of illumination, and six diverse methods of presentation with respect to the subject's left or right eye and with respect to the variable lengths of the adjustable line. The most inconstant aspect of our method consisted in requiring the subject to adjust the figure with his right hand when the variable line was situated at his right and to make adjustments with his left hand when the variable was presented before his left eye

[1] Problem and method were suggested by Crosland, who also supervised the experiment. Statistical procedure and the intelligence data were supplied by Taylor from the Psychological Examination given to all entering freshmen. Newsom has carried out the program and prepared the results.

(both eyes being open during all of the trials). But, as among subjects and as among the three lengths of the variable line, the right and left conditions were kept constant.

Apparatus and Procedure

The Müller-Lyer figure was constructed in black on white cardboard 18 in by 48 in, on which were glued strips of black passe-partout tape, 7/16 in. in width, so arranged as to make a straight line across the center of the white card. Diverging lines of tape 10 cm long were arranged with an angle of 120° between them. This construction gave a standard or non-adjustable open-angle figure 202 mm in length. Another cardboard, sliding freely in grooves, carried the continuation of the main horizontal line and a pair of like, converging wings. The subject adjusted this second cardboard by means of a convenient handle. The settings were read from behind on a millimeter scale. To facilitate judgment and to make it as accurate as possible, we placed at the heads of the angles white paper circles 5 mm in diameter to serve as terminal reference-points.

In order to standardize and to keep constant the conditions of presentation for the subjects and for the several observations of each, the cardboard bearing the figure was situated with the main line horizontal at a distance of one-half meter directly in front of, and on a level with, the eyes. The subject, comfortably seated, could easily move either hand to adjust the movable half of the figure until the two halves appeared equal. The variable was set to the right hand and to the left and apparent equality was reached from plus and minus settings 50 mm from objective equality. In certain series also the variable half was set exactly upon objective equality and then adjusted by the subject. Except for the practice series, all subjects were once given each of these six conditions. The following typed instructions were read:

> The purpose of this experiment is to obtain the judgments of a number of students upon the comparative lengths of these two lines. You are to arrive at this judgment by comparing the distances between the white dots. If you think one of the lines is longer than the other, adjust the adjustable one by this handle until it, in your judgment, just equals the stationary one in length; if you think the two are already equal in length, make no adjustments but state your conclusion to the experimenter. You are to be presented with this task six times, after each of which the experimenter will

make a record of your accuracy, which record, however, will not be revealed to you. Please do not ask to know how well you have done and do not ask about the dimensions of the apparatus.

The subjects were 25 male freshmen in the University of Oregon who, with all other freshmen in the institution, had been tested in the preceding fall by the 1925 Series of the American Council on Education Tests. They were chosen at random. The entire 25 had a range in median percentile intelligence-rank from .05 to .83, with a median rank of .50. The entire group of freshmen tested, 863 individuals, had shown a range from .03 to .99, with a median rank of .49. It is clear, therefore, that the range exhibited in our 25 subjects was not greatly different from the range of the whole group to which they belonged. Two of our 25, who were chosen to engage in practice with the figure, were selected on the basis of taking approximately the same courses in the University, having approximately the same interests, having equally good eyesight, but differing widely (.582 and .050) in median percentile ranks on the intelligence test. It was thought that this difference in rating might appear in the errors of these two subjects with the Müller-Lyer figure and also in whatever effects might come from ten days of practice with the figure under each of the stipulated conditions of presentation. The intelligence rankings of the entire 25 subjects were employed as a basis for obtaining correlation coefficients with the error-score rankings of the same persons in the illusion.

Statistical Procedures

(1) We have instituted comparisons between median percentile rankings in eight typical intelligence tests and the rankings of subjects in millimeter error-scores made in the Müller-Lyer trials, and have obtained Pearson product-moment coefficients, using the average of the errors made under the six conditions for each person as a measure of the amount of the illusion. (2) A similar procedure has been followed with each of the six conditions of presentation. (3) By Huffaker's formula (14) we have obtained inter-correlation coefficients between the error scores under the

six conditions. And finally (4) we have calculated the percentage of error for the entire group of 25 persons as between the six methods of presentation by the formula,

$$\% \text{ of error} = \frac{100\left(\frac{\text{sum } x}{N}\right)}{T},$$

where x equals the values of the individual determinations, N the number of cases (persons), and T the true length of the variable.

Results

1. The general correlation for the 25 subjects between degree of intelligence and the magnitude of error made in adjusting the Müller-Lyer figure was found to be -0.086, with a $P.E._r$. of ± 0.134. Since the probable error here greatly exceeds the coefficient of correlation, one would conclude that these data afford no evidence of a relationship between intelligence ranking and the ranking for susceptibility to the illusion. Had the coefficient been larger, with a reasonably small $P.E._r$., the negative character of the coefficient might have suggested that the subject ranking high in intelligence need not be expected also to rank high in susceptibility to the illusion.[2]

2. The special coefficients of correlation, between intelligence rating and inaccuracy in the judgment of lengths of lines, for the six conditions, are presented in Table I. They are arranged in order from greatest to least correlation.

It is clearly evident that not a single coefficient is as great as its probable error. It would seem, therefore, that, so far as the subjects of this experiment are concerned, no important relationship exists between susceptibility to error in the Müller-Lyer and rank in typical tests of intelligence. Notwithstanding this seemingly justified inference, however, it is an interesting fact that all of the above coefficients are

[2] Pintner and Anderson (27) determined that almost no correlation exists between the intelligence of children and their susceptibility to the illusion; but, under a certain limit, there is a decidedly high correlation of a negative sort with chronological age or growth. This latter fact suggests that maturation, or practice, or both, can account for the gradual diminution of the magnitude of the error as the child becomes older.

negative. That they do not distribute themselves *plus* and *minus* may be regarded as being of some significance. It is not unlikely that a larger number of subjects would have reduced the probable error to a point where some of these negative relationships would have been significant beyond question. The relationships found, however, are too small to be considered of any particular importance even if the P.E.$_r$. were much smaller. Possibly larger numbers would have given larger negative values also.

TABLE I

COEFFICIENTS OF CORRELATION BETWEEN INTELLIGENCE RATING AND INACCURACY SCORES WITH THE MÜLLER-LYER, WITH THE PROBABLE ERRORS (25 SUBJECTS)

Order of Presentation	r	P.E.$_r$.
1	− 0.09	± 0.13
4	− 0.08	± 0.13
3	− 0.07	± 0.13
6	− 0.07	± 0.13
2	− 0.03	± 0.13
5	− 0.01	± 0.13
Average	− 0.086	± 0.13

3. The coefficients of intercorrelation between the error scores in the illusion under the six conditions of presentation were obtained for the purpose of measuring the effects of these various ways of presenting the figure. Huffaker's formula,

$$r_{(1),\,(2)} = \frac{\sigma_1^2 + \sigma_2^2 + (M_1 - M_2) - \dfrac{\Sigma(X_1 - X_2)^2}{N}}{2(\sigma_1\sigma_2)},$$

where $(X_1 - X_2)$ equals the difference between the individual determinations (regardless of sign), and (1), (2) in $r_{(1),\,(2)}$ equals the variable conditions, such as variable right and left, yielded the coefficients which appear in Table II.

The coefficients of correlation shown above (Table II) indicate very definite positive relationships between the Müller-Lyer error-scores and the several conditions of presentation. And inasmuch as the probable error in all cases,

save one, is as small as one-fourth of the coefficient of correlation obtained, the correlations would seem to be definite evidence of more or less similarity [3] between the methods of presenting the figure to the subjects.

TABLE II

INTERCORRELATIONS BETWEEN ERROR SCORES UNDER SIX CONDITIONS OF PRESENTATION, WITH THE PROBABLE ERRORS (25 SUBJECTS)

Presentations Compared	Description*	r (all +)	P.E.$_r$ (all ±)
1 and 2	$R+$ and $L+$	0.61	0.08
1 and 3	$R+$ and $R-$	0.70	0.07
1 and 4	$R+$ and $L-$	0.77	0.05
1 and 5	$R+$ and $R=$	0.59	0.09
1 and 6	$R+$ and $L=$	0.52	0.10
2 and 3	$L+$ and $R-$	0.83	0.04
2 and 4	$L+$ and $L-$	0.36	0.12
2 and 5	$L+$ and $R=$	0.80	0.05
2 and 6	$L+$ and $L=$	0.47	0.11
3 and 4	$R-$ and $L-$	0.81	0.05
3 and 5	$R-$ and $R=$	0.83	0.04
3 and 6	$R-$ and $L=$	0.62	0.08
4 and 5	$L-$ and $R=$	0.53	0.10
4 and 6	$L-$ and $L=$	0.64	0.08
5 and 6	$L=$ and $R=$	0.62	0.08

* "R" and "L" signify variable on right and left; "+" and "−" that the setting of the variable was beyond, and short of, objective equality; and "=" that the setting was at objective equality.

If we arrange these correlation coefficients in order from greatest to least, we obtain the following seriation of comparisons of the conditions of presentation: (1) $L+$, $R-$ and $R-$, $R=$; (2) $R-$, $L-$; (3) $L+$, $R=$; (4) $R+$, $L-$; (5) $R+$, $R-$;

[3] The differences between the r's in Table II may have arisen from chance, *i.e.*, may have no significance. We have taken the trouble to apply the formula, $\frac{D}{\sigma d}$, where σd equals $\sqrt{\sigma r_{(1)} + \sigma r_{(2)}}$ and where $\sigma r_{(1)}$ equals $[(1 - r_{(1)}^2)/\sqrt{N}]$ and $\sigma r_{(2)}$ equals $[(1 - r_{(2)}^2)/\sqrt{N}]$, to the comparisons of three sets of r's in this table. Those chosen were: (*a*) .47 and .36; (*b*) .83 and .36; and (*c*) .62 and .36. The first of these comparisons represents the lowest correlations and the greatest P.E.$_r$'s, presumably giving the least significant differences of r's in the Table. The second comparison is between the highest and lowest r's, presumably giving the most significant difference. And the third comparison is between the median and the lowest r's, presumably showing a difference of fair or median significance. The (*a*) comparison, by the formula quoted, ndicates a significance of 69 : 31 chances out of 100; the (*c*) comparison indicates a significance of 88 : 12 out of 100 chances; and the (*b*) comparison indicates a difference significant in 99.5 chances in 100. Hence we conclude that all the differences of correlation coefficients in Table II are significant, and some much more than others.

(6) *L*−, *L*=; (7) *L*=, *R*= and *R*−, *L*=; (8) *R*+, *L*+; (9) *R*+, *R*=; (10) *L*−, *R*=; (11) *R*+, *L*=; (12) *L*+, *L*=; and (13) *L*+, *L*−. The highest coefficients are hereby shown to obtain under the following conditions: (*a*) when the left hand must adjust toward the subject's right and when also his right hand must be moved to the right; (*b*) when in both instances (of the comparison) the right hand is moved to the right to adjust *V* (the variable); (*c*) when the right is moved to the right and the left to the left; and (*d*) when the left is moved to the right and the right hand also to the right as in (*a*) above. The lowest coefficients obtain when the comparisons relate to (i) movements of the left hand in both instances to the right and then to the left; (ii) movements of the left hand to the right and then to the left as in (i); (iii) movements of the right hand to the left and of the left hand to the left also; and (iv) movements of the left hand to the left and of the right hand to the right. Close observation of *a*, *b*, *c*, and *d*, and i, ii, iii, and iv reveals a conflict, between high and low correlations, with respect to (*c*) and (iv). In both of these cases the left hand is moved by the subject toward his left as he adjusts, and the right hand is moved toward his right. It would seem, therefore, that the high correlation for (*c*) and the low correlation for (iv) could not have arisen from the same factor; namely, reverse movements of the two hands. Closer inspection of the conditions which are compared in these two cases may serve to shed some light upon the question. In (*c*), *V* is in both instances 50 mm shorter than *S* (the standard) and is, moreover, the line enclosed by the converging wings which fact makes it the line which is usually underestimated in length. Any adjustment of it by the subject will therefore be an adjustment which requires movements lengthening it; and if it is set 50 mm shorter than *S*, these movements will be long and extensive, a long one by the right hand and a long one with the left hand; even though these movements are made in reverse directions. In (iv), on the other hand, *V* is to the observer's left and is 50 mm shorter than *S*, requiring a long movement of the subject's left hand to his left; and, in the second instance, is at his right but equal to *S*, thus requiring a short movement of his right hand to his right. The facts here enumerated would suggest a high correlation between two long movements of both hands in reverse directions, and a low correlation between a long movement and a short one of both hands in reverse directions. We shall next inquire if these magnitudes of correlation obtain when the movements are reversed; *i.e.*, when the errors made with a short left-hand left movement are correlated with the errors made with a long right-hand right movement. The comparison of *R*− with *L*= happens to fulfill the condition proposed, and in this comparison a correlation of + 0.62 was obtained. The comparison of *R*= with *L*− (in iv above) also fulfills the condition, and the correlation between them was found to be + 0.53. Both of these coefficients are low, the former representing the median for the 15 coefficients, and there is some difference between them. This difference, we believe, arose from the fact that all of our subjects, save one, were right-handed. And the easier movement of the right hand of the right-handed person is toward his right; it is a question as to what is the easier movement of the right-handed person's left hand, that to his left or to his right. However, it seems clear that the left movements of the left hand of a right-handed person are not as easily effected as are the same movements of the left-handed person.

It was noted that when the variable was set 50 mm shorter than, or equal to, the standard, the subject would invariably move *V* to a position where it was obviously longer

than *S* and would then make short reverse, corrective movements readjusting *V* until it finally appeared to equal *S*. These reverse, corrective movements of either hand occurred most frequently and with the largest number of fine readjustments when *V* was shorter than *S*. They occurred less frequently and less conspicuously when *V* was equal to the standard. It is not impossible that the inconsistencies of the coefficients for the errors made under the six methods of presentation were related to the differences and similarities between the methods of presentation arising from these complex and subtle movements and reverse movements. It is also not impossible that there is a significant correlation between handedness and eyedness, and that the differences between the two eyes, even though they were used binocularly in the trials, had an influence on the error-scores when *V* was presented on the right and on the left of the field of vision.[4]

We shall make another attempt to explain the differences present in the coefficients of Table II by regrouping the coefficients in a way which will compare *R*'s with *L*'s, −'s with +'s and with =' s, and so on. This regrouping is shown in Table III.

TABLE III

REARRANGED CORRELATION COEFFICIENTS BETWEEN ERROR SCORES UNDER SIX CONDITIONS OF PRESENTATION

R's		*L*'s	
Comparison	r (all +)	Comparison	r (all +)
R− and *L*−	0.81	*L*− and *R*−	0.81
R− and *R*=	0.83	*L*− and *R*+	0.77
R− and *L*+	0.83	*L*− and *L*=	0.64
R− and *R*+	0.70	*L*− and *R*=	0.53
R− and *L*=	0.62	*L*− and *L*+	0.36
R+ and *L*−	0.77	*L*+ and *R*−	0.83
R+ and *L*+	0.61	*L*+ and *R*=	0.80
R+ and *R*=	0.59	*L*+ and *R*+	0.61
R+ and *L*=	0.52	*L*+ and *L*=	0.47
R= and *L*+	0.80	*L*= and *R*=	0.62
R= and *L*=	0.62	*L*= and *R*−	0.62
R= and *L*−	0.53	*L*= and *R*+	0.52

[4] To lend weight to this supposition, Valentine's (39) interesting discovery that differences between the two eyes do have an influence in causing variations in error-scores in the horizontal vertical illusion may be cited.

The median coefficient presented above is + 0.62. If we count the number of coefficients (Table III) which involve an *R*− and which also exceed the median, we shall discover that there are four such; *i.e.*, 80 per cent. of the *R*−'s are involved in coefficients which are greater than the median. Likewise, we find that only 40 per cent. of the *R*+ coefficients exceed this median, and only 40 per cent. of the *R*= coefficients exceed it. Similarly, 60 per cent. of the *L*− correlations and only 40 per cent. of the *L*+ coefficients and only 20 per cent. of the *L*= exceed the median of 0.62. These percentages incline us to conclude that the *R*− and *L*− conditions of *V* contained common elements within themselves and common elements with other conditions of *V*, so that, when the errors obtained from them are compared, a high correlation results; and that *plus* and *equal* conditions of *V* with *S* contained such dissimilar elements as to be responsible for much lower correlations.

Of only the four cases of either *R*+ or *R*= being involved in a comparison leading to a correlation higher than the median, thrice or 75 per cent. of the times the comparison also involved an *R*− or an *L*−. Of the three cases of either *L*+ or *L*= being involved in a comparison leading to a coefficient greater than the median, twice or 67 per cent. of the times the comparison also involved an *L*− or an *R*−. Hence, for the obtaining of a high correlation, the clear advantage would seem to lie with the *R*− and *L*− conditions of presenting *V* and the clear disadvantage would seem to lie with the *L*+ and the *L*= conditions of *V* when compared with the *R*+ and the *R*=.

4. Our fourth statistical procedure, the calculating of the percentage of error for the entire group of 25 persons under each of the six methods of presenting the Müller-Lyer figure (employing the formula in a foot note on page 45), has furnished the results which appear in Table IV.

It is to be noted in the foregoing table that the conditions of *V* as *plus* (greater than *S*) led to a greater degree of inaccuracy in the subjects than did *minus* or *equal* conditions.

Although this result was unexpected, we believe that it arose from the fact that when *V* was longer than *S* the subject invariably moved the adjustable slider carefully to the region where equality was apparent and thereupon ceased his efforts in further readjustments of *V* and did not employ reverse movements as he had done when *V* was shorter than *S*. It is obvious also that the latter condition of *V* (shorter than *S*) was accompanied by the most accurate adjustments, a result which was wholly unexpected. The variable, being enclosed by converging wings, is usually under-

estimated; and, in this case, where it was set to be actually shorter than *S* it would have been reasonable to suppose that the underestimation would be increased and the inaccuracy magnified. Such, however, was not the case; the *minus* conditions of *V* made the judgments more accurate. Since this result is opposite to the one accruing from *plus* and *equal* conditions of *V*, it is to be explained by an appeal to ther eversal movements and the more frequent movements which the subject made when he attempted to adjust minus *V*'s to make *V* appear equal to *S*. That this explanation is probably correct is attested by the fact that the equality conditions of *V* also led to very accurate adjustments, these adjustments being also characterized by reversal movements of the observer's hands. One other fact in Table IV stands out; the *L* adjustments were more accurate than the *R* adjustments. This result we are unable to explain, unless we suppose that the assumed left-eyedness of right-handed subjects made their judgments more accurate when the variable and adjustable line was presented almost directly in front of the left eye.

TABLE IV

ERRORS FOR THE GROUP UNDER SIX CONDITIONS OF POSITION AND EXTENT OF THE VARIABLE (25 SUBJECTS)

Order of Presentation	Description of Presentation	% of Error
1	*R*+	17.3
2	*L*+	17.1
3	*R*−	15.7
5	*R*=	15.5
6	*L*=	14.0
4	*L*−	13.6
1, 2	Sum of +'s	16.2
1, 3, 5	Sum of *R*'s	16.1
2, 4, 6	Sum of *L*'s	14.9
5, 6	Sum of =''s	14.8
3, 4	Sum of −'s	14.7
By averages of all conditions		15.5

In general, the average percentage of error of our subjects with the Müller-Lyer figure approximated that obtained by Seashore (33). His results showed a score of 21 per cent. of inaccuracy, while ours show only 15.5 per cent. This small difference is probably to be explained by the fact that he employed in his Müller-Lyer figure wings at an angle of 60°, while the wings of our lines made angles of 120° between themselves. It is well known that the more the wings approach vertical positions, the less the illusion,—a fact which one can easily demonstrate for oneself in the perceiving of the Jastrow (15) variation of the standard figure.

Summary

We have found with our 25 subjects (1) no significant correlation between intellectual rating and efficiency of comparison and judgment in the Müller-Lyer illusion; (2) a slight suggestion of a possible negative relationship between high intelligence and high inaccuracy in the illusion; (3) a definite indication of this negative relationship as resulting from the practice of two persons with the figure; (4) definite practice effects in two subjects, the only two in whom practice was investigated; (5) a high correlation between some of the modes of presentation of the figure and lower correlations between certain other modes, these entailing methods of the observer's own in adjusting the variable to appear equal in length to the standard line. It also appears (6) that *minus* lengths of the variable line led to the most accurate judgments, *plus* and *equal* lengths to the most inaccurate and that (7) adjustments made with the left hand were more accurate than those made with the right.

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THE INFLUENCE OF OCCUPATION UPON THE PERCEPTION OF TIME

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This study is an attempt to find out how unlike 'situations' influence the perception of time. The subjects were 326 members of eight laboratory sections in an introductory course in psychology, each section containing from 35 to 45 students. Each section was asked to estimate the length of each of eight periods during which the subjects were engaged in eight markedly different types of activity. In order to facilitate computation and to make the results more justly comparable, each situation lasted 200 seconds. To prevent the students from suspecting this common duration, a period of 260 seconds was introduced between the second and third periods, and another period of 100 seconds between the fifth and sixth periods. To correct for any constant influence due to the position of the situation in the series, the situations were given in the same order to each of the eight sections, but commencing each time with a different situation, so that each was, in turn, once first, once second, and so on.

The following directions were given each section.

It is a well-known fact that under some conditions time seems to pass very rapidly, while under others it seems to pass very slowly. The object of these experiments is to find out just what conditions make time pass quickly and what conditions make it pass slowly.

In order to do this you will be asked to occupy yourselves in different ways during different periods of time, and then to estimate the length of the period, recording your estimates in seconds on cards that will be passed out to you. [Here cards were passed out.]

Do not estimate the time by counting, or beating time, or marking off seconds. Simply forget all about estimation during the period, and enter whole-heartedly into the work. Then afterwards look back and estimate the time.

The interval to be judged will be marked by my saying "go" at the beginning of the interval, and "stop" at the end. We are now ready to begin the experiment.

The eight situations used can be best described by quoting the instructions given to the class at the beginning of each situation.

1. *Rest.* Put your arms on the table and lay your head on them, but keep your eyes open and remain attentive. When I say "go" shut your eyes and relax completely. Do not count or mark time, remain perfectly passive, as if trying to sleep. When I say "stop" sit up, estimate the time and record it.

2. *Rapid Metronome.* (184 beats a minute.) A metronome will be started ticking; sit quietly and listen to the metronome. It will not be necessary to say "go" or "stop" in this test; just estimate the length of time the metronome was ticking. But do not count the ticks.

3. *Slow Metronome.* (66 beats a minute.) Instructions same as above.

4. *Division.* These sheets contain some simple problems in long division. When I say "go" start working the problems. When I say "stop" stop working and estimate the time.

5. *Mirror.* A mirror will be passed to each one of you, a shield, and a card containing some simple directions. The card will be placed in position so it will not be necessary to turn it. Leave it as laid on your desk. Look at the reflection of the card in the mirror and read the directions, and follow them as you read. Start reading in what appears to be the lower left-hand corner of the card, as this is the place where the first line actually begins. Read the directions in the mirror and follow them as you read.

6. *Pain.* Do not touch these boards until directed to do so. In the center of each one you can see a small point. It is very short and cannot stick into the flesh. When I say "go" put the heel of the palm over this point, and press down until you feel the point. Press until you get a sensation of pain, and keep pressing until I say "stop."

7. *Fatigue.* When I say "go" extend the arms to the side with the palms upward and hold them there until I say "stop"; then estimate the time.

8. *Dictation.* [A passage from the text in psychology far in advance of their assignment was used.] During this interval you will write as I dictate on these sheets I am passing out. It will not be necessary to say "go" or "stop." I will simply start dictating, and when I stop you will estimate the length of time I have been dictating.

Results

The following table shows the final results pooled for all the laboratory sections, giving the means in their rank-order together with their sigmas.

Situation	Ave. Estimate (sec)	Sigma of Estimate (sec)
Rest	241.7	107.8
Fatigue	228.4	96.2
Slow Metronome	223.7	92.4
Rapid Metronome	214.1	85.2
Pain	210.2	78.4
Mirror	181.8	77.6
Dictation	174.6	77.4
Division	168.9	70.2

In the next table, the first figure in each group of three is the difference between the means of the two situations, the second is the sigma of that difference,[1] and the third is the number of times in 1,000 such a difference and sigma would occur by pure chance.

	Rest	Fatigue	Slow Metronome	Rapid Metronome	Pain	Mirror	Dictation
Division	72.8	59.5	54.8	45.2	41.3	12.9	5.7
	5.58	6.8	5.13	5.53	5.77	4.29	4.41
	$1/10^{35}$	$1/10^{12}$	$1/10^{20}$	$1/10^{12}$	$1/10^{9}$	.0013	98
Dictation	67.1	53.8	49.1	39.5	35.6	7.2	
	5.28	6.04	4.95	5.23	5.38	4.61	
	$1/10^{30}$	$1/10^{12}$	$1/10^{20}$	$1/10^{9}$	$1/10^{6}$	59	
Mirror	59.9	46.6	41.9	32.3	28.4		
	5.78	6.57	5.37	5.61	5.28		
	$1/10^{20}$	$1/10^{9}$	$1/10^{9}$	$5/10^{5}$	$3/10^{5}$		
Pain	31.5	18.2	13.5	3.9			
	5.66	4.15	4.76	5.21			
	$2/10^{5}$	.0057	2.33	227			
Rapid Metronome	27.6	14.3	9.6				
	5.35	5.61	4.12				
	$12/10^{5}$	5.55	10				
Slow Metronome	18.0	4.7					
	5.15	5.35					
	.225	190					
Fatigue	13.3						
	5.92						
	12						

Do the above figures reveal only the influence of the situation upon the perception of time, or are they affected by other factors which are concealed but nevertheless have an influence upon the results? It is possible to isolate one or two rather obscure factors and to test for their influence upon the results. Let us consider two such possible factors; namely, the influence of the position of the estimate in the series (as first, third, *etc.*) and the general average of the subgroup (laboratory section).

[1] The sigma of the difference was computed by the formula

Sigma of the difference between mean of x and of y

$$= \sqrt{(\text{sigma of mean of } x)^2 + (\text{sigma of mean of } y)^2 - (2r_{xy})(\text{sigma of mean of } x)(\text{sigma of mean of } y)}.$$

The statement of chance occurrence was taken from Pearson's Tables for Statisticians and Biometricians.

Did the position of the estimate in the series have any influence upon the actual estimate? A rough answer to this question was obtained by using the sixty-four means (eight in each of the eight sections) and correlating the position with the actual amount of the estimate. The resulting correlation was very low, .081. This would seem to show that the position of the situation exerted a negligible influence upon the estimate. This should not be interpreted, however, to mean that position never makes a difference. There were only eight situations in the experiment; not enough, possibly, to bring this factor into prominence.

Another factor which might have affected the results was the great length of the estimates of some sections and the shortness of the estimates of others. Was this difference enough to influence the results? To answer this question the sections were ranked according to the mean of all their estimates, and this ranking correlated with the mean estimate of each section for each situation. The correlation was found to be .449.

Then in order to see how much influence the actual situation had on the estimate the means were again correlated. This time the average estimate of each situation by each section was correlated with the average pooled estimates for all sections. Knowing the average estimate of the situation for all sections combined, this procedure should enable us to predict the average estimate of any single section for that situation. A coefficient of .763 was obtained.

These correlations worked out with means would seem to show roughly that the position was negligible. The general tendency of an individual section to estimate the times as all long, or all short, had too much influence to be desirable; but the situation itself was the most important factor.

The partial correlation between estimate and situation—partialing out the influence of general tendency of an individual section to estimate all times as long or all times as short, and also partialing out the influence of the position in the series—was then worked out. This partial correlation coefficient was found to be .858, with a sigma of .033. This shows that the relation was really closer, and that the differences found are probably more significant than they appear to be.

The relative influence of the three factors of position, section, and situation upon the actual estimate was also brought out by getting the regression equation. Computed for standard scores it was:

$$\text{Estimate} = .763 \times \text{Ave. of situation} + .449 \times \text{Ave. of section} + .081 \times \text{position in the series.}$$

This also shows that the situation exerted the greatest influence; that differences between sections were a little larger than desirable, and that the effect of position was negligible. The accuracy of the regression equation was also tested by actually making the predictions and working out empirically the multiple correlation, and then working it out by formula. The empirical multiple correlation coefficient was .892, with a sigma of .026. The multiple correlation

coefficient worked out by formula was .889, with a sigma of .026.

These results justify the opinion that the actual differences are even more significant than the obtained differences, but just how much more significant cannot be determined.

Sex Differences

The data were in such form that differences presumably due to sex could easily be determined. These differences in both means and sigmas, together with the sigma of the difference of the means, and the sigma of the difference of the sigmas, are given below.

Situation	Females Mean	Males Mean	Diff'ce of Means	Sigma of Diff'ce	No. Times in 1,000 Diff'ce Would Occur by Chance Alone
Rest	255.1	229.7	25.4	10.6	8.5
Rapid Metronome	225.4	203.8	21.6	9.4	11
Slow Metronome	235.1	213.3	21.8	8.6	6
Division	172.6	165.6	7.0	8.6	208
Mirror	186.6	177.5	9.1	8.6	145
Pain	226.1	195.9	30.2	10.2	2
Fatigue	238.9	199.8	39.1	12.0	0.7
Dictation	176.5	172.8	3.7	7.8	318

Situation	Females Sigma	Males Sigma	Diff'ce of Sigma	Sigma of Diff'ce	No. Times in 1,000 Diff'ce Would Occur by Chance Alone
Rest	96.8	94.0	2.8	7.4	352
Rapid Metronome	89.4	79.8	9.6	6.6	73
Slow Metronome	85.2	68.2	17.0	6.0	2
Division	80.6	76.0	4.6	6.0	222
Mirror	85.0	69.2	15.8	6.0	4
Pain	102.4	79.4	23.0	7.2	0.7
Fatigue	129.4	78.8	50.6	8.4	0.0003
Dictation	72.2	68.0	4.2	5.4	219

The women's estimate was significantly longer than the men's, except on the situations of long division, mirror reading, and dictation. The sigma of the women is always greater than that of the men; but the difference in relative variability may not be so striking, because there is a tendency for the sigma to increase as the means increase. Hence if

the mean of the women is greater than that of the men one would expect the sigma to be larger. In order to compare the variability of the women with that of the men the Pearson coefficient of variability (100 × sigma ÷ mean) was worked out. Differences and sigmas of differences together with a statement of the number of times in 1,000 such differences would occur by pure chance are as follows.

Situation	Females Coef. of Var.	Males Coef. of Var.	Diff'ce of Coef. of Var.	Sigma of Diff'ce of Coef. of Var.	No. Times in 1,000 Diff'ce Would Occur by Chance Alone
Rest	37.946	40.923	− 2.977	3.08	167
Rapid Metronome	39.663	39.156	+ .507	3.09	435
Slow Metronome	36.240	31.974	+ 4.266	2.69	57
Division	46.698	45.894	+ .804	3.63	412
Mirror	45.552	38.986	+ 6.566	3.34	25
Pain	45.290	40.531	+ 4.759	3.38	80
Fatigue	54.165	39.439	+14.726	3.74	0.05
Dictation	40.906	39.352	+ 1.554	3.15	311

It can be seen from this table that differences in relative variability of men and women in their estimates of time are not very great for most of the situations used, the situation called "fatigue" being the only notable exception.

These sex differences in both mean and sigma, although apparently not all significant, are of very great interest because the same tendency of women to have larger means and sigmas than men has been found by several other investigators.

MacDougall[2] found that, in the four situations which he studied, the sigma of the women was always greater and the mean also greater; and Yerkes and Urban[3] declared that "without exception the means for the females are longer than those for the males" and that "the mean variability is very much greater for the females as is also the relative variability." This study was made with the time intervals 18, 36, 72 and 108 sec. One statement occurring in the study of Yerkes and Urban seems to indicate that if the interval is shortened to one second the relation is reversed. "The length of a second is slightly overestimated by men, and greatly underestimated by women. Intervals of 18–108 seconds are usually slightly underestimated by men (17–23 years) and greatly overestimated by women (17–20 years)." Upon investigating the methods used, however, it is at once apparent that this seeming reversal with very short times is due merely to a reversal of the use of the terms 'overestimate'

[2] R. MacDougall, *Science*, 1904, **19**, 707.

[3] R. M. Yerkes & F. M. Urban, *Harvard Psychol. Stud.*, 1906, **2**, 427.

and 'underestimate.' In the case of the 18 to 108 sec intervals, the time is said to be overestimated if the subject thinks the interval is longer than it really is, while in the one-sec interval it is said to be overestimated if the subject thinks a second has elapsed when in reality more than a second has elapsed. So this apparently curious reversal of the position of men and women when very short intervals are considered is due merely to a confusion of terms.

Swift and McGeoch[4] have recently published a study which shows slighter differences in time-estimation due to sex. I have worked out from their figures the sigma of their sex differences, translated it into standard scores, and found from the tables of probability integrals how many times such a difference would occur by chance. These results are given in the following table, where the minus sign before the difference indicates that the men's estimate was larger than the women's and the plus sign that the women's estimate exceeded the men's.

Female Mean	Male Mean	Diff'ce of Means	Sigma of Diff'ce of Means	No. Times in 1,000 Diff'ce Would Occur by Chance Alone
12.32	12.20	+.12	.388	379
9.62	9.65	−.03	.311	462
9.10	9.29	−.19	.345	291
9.18	8.50	+.68	.353	27
6.20	7.04	−.84	.343	7
6.30	6.50	−.20	.347	282
3.18	3.195	−.015	.209	472
3.38	3.48	−.10	.234	335
1.853	1.869	−.016	.145	456
1.545	1.47	+.075	.082	180
0.738	0.69	+.048	.045	143
0.783	0.833	−.05	.056	186

These results are very puzzling. In eight of the twelve tests the men judged the time longer than the women. Of these eight only two are, however, sufficiently great not to have been chance differences; while in the four tests where the women's estimate was longer it is probable that three are sufficiently great to be significant. The difference which was least likely to occur by chance came where the men's estimate was longer. This study shows a similar lack of uniformity in sex differences in variability, whether measured in sigma, or by the Pearson coefficient of variability. For some situations the women were more variable, for others the men. No general tendency in any direction is shown.

The studies of MacDougall and of Yerkes and Urban, together with the present study, seem to indicate that there is a fairly strong probability that women will estimate a given interval of time as longer than the men's estimate. The study of Swift and McGeoch does not concur. As regards sex differences in variability there is not much agreement, the probabilities being that women are slightly more variable in their estimates than are men.

[4] E. J. Swift & J. A. McGeoch, *J. Exper. Psychol.*, 1925, **8**, 240–249.

Summary and Conclusions

The present study supports the following conclusions.

1. Within a limited range, differences in the estimation of time depend primarily upon the way in which the individual is occupied or employed. The eight situations used ranked in the following order from longest to shortest: complete rest, holding arms extended from the side, listening to a slow metronome, listening to a rapid metronome, holding the palm on a thumb tack, reading directions in a mirror, copying from dictation, doing long division.

2. Differences in estimation as between one laboratory section and another exert more influence upon the estimate than do differences of position of the situation in the series, at least when the series is short.

3. There are marked sex differences in the estimation of time, the women tending to estimate durations as longer. Other investigators also found this difference.

4. Sex differences as regards variability are not so marked, especially where the Pearson coefficient of variability is taken as the true measure of variability.

The Accuracy of Timing with the Stop Watch

By Eldon K. Rumberger

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How accurately do timekeepers commonly record time with the stop watch? Suppose that an athlete runs the hundred-yard dash in nine and three-fifths seconds recorded time. Is this recorded time accurate? If it is not, how great is its inaccuracy? It was the aim of our experiment to answer these questions.[1]

The subjects were taken singly and in pairs. The reason for testing in pairs was to ascertain how often the two subjects would agree in their times. In timing athletic events the time reported by a majority of the timers is usually taken. If two timers out of three report the same time, that time is considered official. If there is no agreement, the highest time is taken. It is a rule, at some places, to take the intermediate time. To my knowledge the average time is never used.

The timing subject started and stopped his watch at two successive appearances of a red signal which filled a small square hole in a gray screen. Behind the screen the instant of the red appearance was mechanically recorded upon a spirally moving kymographic drum. The drum also carried a time-line from a 50 d.v. fork. The extreme range of error in recording the exposure of the red signal was estimated at 5σ. The subject was instructed to start his stop watch upon the first appearance of the red signal and after he had heard the preparatory words "Ready: get set." At the second appearance, he was to stop his watch without further 'ready' signals. Preliminary trials acquainted him with the operation. Observations were made to the number of 435 with 11 subjects, 7 of whom were expert in the use of the stop watch. The actual times ranged between 2.5 sec and 11.2 sec. Our fork records could be accurately read to 5σ (1/4 vib.). From 15 to 20 times were taken at a sitting. The following sample of our data sheets will show how we kept the records. The column "A.T." holds the actual times, as these were taken from the drum: "S.W.T." is the subject's stop watch time; while "D.O.A." and "D.U.A." give respectively overestimations and underestimations of the tuning fork (the "actual times").

Trial	A.T.	S.W.T.	D.O.A.	D.U.A.
1	5,325	5,400	75	
2	7,165	7,300	135	
3	9,245	9,200		45

Representative values, as taken from the results, follow. They include, first, the total range of "error," *i.e.*, the departure from the 'actual' time (as recorded by the tuning fork) in the stop watch records made by the several subjects. They distinguish, secondly, overestimations and underestimations of the 'actual' times.

[1] The work was done in the Psychological Laboratory under the direction of Dr. J. E. DeCamp.

They indicate, further, the relative number of errors which exceeded 1/5 sec (the common unit of reckoning on the field of sport); and they conclude with the maximal error made by any individual in the 'expert' and the unskilled groups. The lower and upper limits of range are given in the table.

RANGE BY INDIVIDUAL SUBJECTS

For seven 'expert' time-keepers: 240 trials
(Times given in sigma)

Ave. error, by individuals	from 84	(lowest) to	196	(highest)
Mean var'ns, by individuals	from 51	(lowest) to	124	(highest)
Ave. of 156 overestimations	from 8	(lowest) to	211	(highest)
Ave. of 84 underestimations	from 31	(lowest) to	137	(highest)
Per cent. errors exceeding 1/5 sec	from 7%	(lowest) to	47%	(highest)
Highest single error			550	

For four individuals unskilled in timing: 195 trials

Ave. error, by individuals	from 82	(lowest) to	261	(highest)
Mean var'ns, by individuals	from 55	(lowest) to	145	(highest)
Highest single error			1,010	
Per cent. errors exceeding 1/5 sec	from 26%	(lowest) to	52%	(highest)

It appears from the results that the most accurate individual in the 'expert' group erred by more than 1/5 sec about once in 14 trials (7 per cent.), and the least accurate individual in nearly half his trials (47 per cent.). The corresponding gross errors of the unskilled were about 1 in 4 (26 per cent.) and 1 in 2 (52 per cent.). The largest single error in the first group was over 1/2 sec (550σ), and in the unskilled over 1 sec (1,010σ). The poorest of the 'experts' erred, on the average, almost 1/5 sec (196σ); the poorest of the unskilled, more than 1/4 sec (261σ). It appears, then, that, under the conditions of the experiment, all of the subjects made errors of the order of the unit of measurement in field timing, and that some 'expert,' as well as all unskilled, individuals frequently made such errors. The comparatively large mean variations also suggest high variability and small degree of certainty in timing brief events under these conditions. While it cannot be urged that the errors of our small experiment are exactly duplicated in general practice, it seems highly probable that our conditions within the laboratory are more favorable to accurate timing than the grosser conditions upon the field of sport.

The Fixational Pause of the Eyes

By David L. Bidwell

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Cobb and Moss (1) have recently sought to determine by a new method the duration of the pause for fixation in the ordinary use of the eyes. The new method promised to eliminate certain difficulties inherent in the photographic method and in direct attachment of a recording system to the eye-ball. The procedure was carried through with great cleverness and skill. To the present writer, however, it seems to be subject to criticism in two or three important respects. First, the authors' preliminary analysis of the experimental situation seems to have been at fault; secondly, the investigators failed to secure certain relevant facts, and thirdly, the matter of control appears not to have been adequately dealt with.

McAllister (2) has demonstrated that during the "fixational pause" (as it is called) the eye is not really at rest, but is continually moving in small excursions of varying lengths. These are said by Dodge (3) to serve the purpose of substituting fresh for fatigued retinal areas. These excursions vary in length, and, together with the initial movements of fixating the exact spot (after the sweep), they tend to preclude the assumption of a single, durable pause. In fixating a point, it is not usual for the eye to move to it with great precision. Returns and corrective movements are the rule rather than the exception.

The preliminary analysis of the situation failed further in not considering visual perception. There are many visual illusions of motion produced by successive stimulations. There is the whip-lash illusion, for example, where the perception of motion extends beyond the actual point stimulated. Again, in a recent study of movements perceived from the region of the blind spot, Miss Stern (4) found that the illusory motion was frequently extended beyond the actual point of stimulation. Wertheimer and Higginson might also be cited. These are examples which naturally bear upon the present point. It is obvious that Cobb and Moss's successive stimuli were not sufficiently separated in time to exclude the positive after-image as well as the perception of motion. No reports are included to indicate the nature of the actual perception. The subjects may not have seen a discrete series of flashes at all. They may simply have guessed the length of a continuous arc; and, if the pause was not fixed and steady, they may have inferred the number from the position of the ends. This seems to be an example of the pitfalls which await those who reject everything savoring of "introspection." In such an experiment there are, as a rule, certain factors which can be secured in no other way than through the strictly controlled *report*. In this experiment there was needed a series of 'control' observations. The stimuli should have been so regulated that the arc illuminated by the flashes was defined at both ends and the subject's report compared with the conditions of visual excitation.

The method should have included also a photographic check to control the errors from eye-movement. It was needed, too, for the purpose of establishing a new and presumptively valuable technique. With these controls and with the reports of trained observers most of the points at issue could be settled.

Cobb and Moss have accidentally reversed the relation between horizontal and vertical reading as reported by Miles and Shen (5). The true difference is slightly longer for vertical than for horizontal alignments, instead of the 20 to 30 sigma difference in the reverse direction, which they attribute to these authors.

Again, one may ask whether flashes of light arranged in circular form and otherwise meaningless are comparable to such patterns as letters or forms, and whether they fulfill the conditions specified as the "ordinary use of the eyes." Can one generalize from a circle of lights to office or factory surroundings? Or to general illumination at various brightnesses?

It is regrettable that methods could not have been used which should have justified the new method before it was launched as a means of research. As it stands, our knowledge of methodology is but little advanced.

To sum up, our objections are based on the following points: first, faulty analysis; secondly, failure to secure certain relevant data; and thirdly, inadequacies in the matter of control. For these reasons the cogency of the conclusions is seriously impaired.

References

1. P. W. Cobb & F. K. Moss. The fixational pause of the eyes, J. Exper. Psychol., 1926, **9**, 359–367.
2. C. N. McAllister. The fixation of points in the visual field, *Psychol. Rev. Monog. Suppl.*, 1905, **7** (no. 1), 17–55.
3. R. Dodge. An experimental study of visual fixation, *Psychol. Rev. Monog. Suppl.*, 1907, **8** (no. 35).
4. K. Koffka & A. Stern. Die Wahrnehmung von Bewegung in der Gegend des Blinden Flecks, *Psychol. Forsch.*, 1925, **7**, 16–44.
5. W. R. Miles & E. Shen. Photographic recording of eye movements in the reading of Chinese in the vertical and horizontal axes, J. Exper. Psychol., 1925, **8**, 344–362.

A Note on Auditory Theories

By Ralph H. Gundlach

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Professor Boring has recently[1] placed all students of audition under heavy obligations by bringing together the facts on tonal intensity, volume and localization and by relating them to the 'place' theories and the 'frequency' theories of audition. "Of straightforward attempts to bring the known facts into reasonable relation, to point out clearly the lacunæ in our knowledge and the discrepancies that remain in even the best interpretation, and to formulate the crucial experiments that might resolve these discrepancies" Professor Boring finds but little in the literature; and it is precisely these defects and deficiencies which his own article goes far to remove. Although the present writer has, at the moment, little to add by way of further construction or of theoretical extension, there are, in the exposition, certain specific points which may possibly profit by critical discussion. We take them in order. The first two concern volume and intensity, the third localization, and the last is a matter of omissions.

i. Boring remarks that, under the frequency theory, intensity may be correlated with the number of specific fibers stimulated (159) and that the separate impulses may be cortically integrated (160), somewhat as the impulses from separate motor fibers are integrated in the contracting muscle. But there are certain experimental facts which stand against such an intensive summation. The intensity of a tonal triad, *e.g.*, is not the sum of the individual intensities, as Stumpf[2] has shown; it is no stronger than the intensity of the strongest component. Likewise, the intensity of a number of simultaneously stimulated cold organs is, as Baron and Bentley found,[3] no higher than that of the most highly tuned organ excited. Once more, in the field of visual phenomena, two lights of unlike radiant energy do not, when 'mixed,' summate; the resultant falls instead under Talbot's Law. Nor do contrast effects involve the summation of the 'contrasted' intensities. If the neural response is all-or-nothing and intensive increase derives only from spread, it is curious that plural tones, plural lights, and plural colds—which presumably arise under well-integrated neural conditions—should not exemplify this summational excitation. The functional difference between the assumed neural spread (under the all-or-nothing hypothesis) and the co-excitation of near-lying receptors would not seem to be sufficient to account for intensity and volume unless one is inclined to rely upon specific energies much more heavily than Boring appears to do.

[1] E. G. Boring, Auditory theory with special reference to intensity, volume and localization, *Amer. J. Psychol.*, 1926, **37**, 157–188.

[2] C. Stumpf, *Tonpsychologie*, 1890, ii, 423 f.

[3] A. Baron & M. Bentley, The intensive summation of thermal sensations, *Psychol. Monog.*, 1921, **30** (no. 136), 84–94. See also S. E. Barnholt and M. Bentley, Thermal intensity and the area of stimulus, *Amer. J. Psychol.*, 1911, **22**, 325–332.

ii. It is possible that Boring here confuses volume and intensity, since the stimulus for both is the amplitude of the air-wave (162, 164),[4] and since the two are not physiologically differentiated with clearness. In *monaural* function, volume and intensity are made dependent upon the number of fibers stimulated (160 f, 164); while in *binaural* function, volume and intensity both depend upon the degree of a central dispersion (167, 174 f). Although at times Boring appears to consider volume and intensity as in some manner physiologically independent (175),[5] they would really seem to be distinguishable only in experience and on the basis of different limens (162).

iii. Localization is said to depend on such an overlapping of 'binaural' cortical areas that the side which leads in intensity, phase, or time shall inhibit, in the common area, the effects of stimulation at the other ear (167 ff). Why then—may we ask—are monaural sounds localized at all?[6] Let us return, however, to binaural hearing. Here localization must be dependent upon the assumed cortical inhibition, which should cause a variation in the volume of the diotic sound (173). Halverson found such a change in volume for differences in phase;[7] but the change was not continuously in one direction. Although volume was found to decrease when localization based on phase differences passed from the median plane to one side, it reached a minimum "somewhere between 1/8 and 1/4 of a wave length, and for greater phase-differences the volume increased again" (174). There are, then, on each side of the median plane two positions where the volume is the same, within the range from minimal to greatest lateral volume. If localization is based on differences in volume, confusions of these positions should have been found. Since this is not the case, localization would not seem to be entirely dependent upon differences in tonal volume. Boring's own explanation (175) does not consider this point since, at the moment, his own interest is not so much in the problem of localization as in showing how the changes in volume could occur on the theory of overlapping areas.

iv. Boring asks for a direct attack on the possibility of reducing phase-differences to differences in time (172). He does not mention Banister's recent work on this question.[8] Banister used seven observers on about twenty different frequencies between 133 dv and 1,705 dv and found no tendency on the part of any observer (except himself) to change localization for a phase-difference of $\pi/2$ in the sounds at different frequencies. Finally, Boring has overlooked attributive differences[9] of brightness among tones, which may be as important for localization as tonal volume or intensity.

[4] Although differences in intensity depend upon differences in the amplitude of the air-wave for the same tone, as Halverson found (*Amer. J. Psychol.*, 1924, **35**, 360–367), the relation between intensity and amplitude is more doubtful for different pitches. Wien (*Arch. f. d. ges. Physiol.*, 1903, **97**, 1–57) and M. Guernsey (*Amer. J. Psychol.*, 1922, **33**, 554–569) found the greatest sensitivity to intensity around 3,200 dv to 3,800 dv. The amplitude of low tones of the same energy would be much larger.

[5] *E.g.*, "Tonal volume should depend upon the phase-relations of a diotic frequency" (186); but in such a case monaural sounds would have no volume.

[6] *Cf.* A. H. Pierce, *Studies in space perception*, 1901, pt. I, chap. 3.

[7] H. M. Halverson, Diotic tonal volumes as a function of difference of phase, *Amer. J. Psychol.*, 1922, **33**, 526–534.

[8] H. Banister, Localization of tones at various frequencies, *Brit. J. Psychol.*, 1924–25, **15**, 280–307.

[9] O. Abraham, Zur physiologischen Akustik von Wellenlänge und Schwingungszahl, *Zsch. f. Sinnesphysiol.*, 1920–21, **51**, 121–152. *Cf.* R. M. Ogden, *Hearing*, 1924.

By emphasizing the facts of volume, intensity and localization, Boring has made it evident that many of the experiments formerly considered as supporting the resonance theories are open to equivocal interpretation. He has, moreover, formulated a large number of experimental problems whose solution promises to go far towards establishing an informing and tenable theory of hearing. The stubborn and basal fact of phenomenal analysis still confronts us. Boring guesses that "some factor of resilience, perhaps in the receptor," may be found and made to replace the selective resonance of Helmholtz and others. Let us hope that the ample experimental program now before us will lead in time through "theorizing" to "theory."

A Simple Class Demonstration of Apparent Movement

By Glenn D. Higginson

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The writer has recently devised a means of presenting in succession and under temporal control two resting stimulus-objects of variable light-intensity for the creation of phenomenal movement.[1] The chief factor involved was the presentation of the second object at such an intensity that it instantly cancelled from the perceptual field the first, weaker object, which nevertheless remained in the physical sense. Under these simple conditions very clear and insistent movement was observed from the first to the second object. The following adaptation of this device has now been made for demonstration before large classes at the University of Illinois.

Four or five feet before a large white screen (used for lantern projections) unrolled behind the lecturer's desk, a long slender upright rod was set in an ordinary triangular base upon the floor. Two 75-watt lamps, screened from the audience by 'student' shades, were set upon the desk in such positions as to cast shadows of the rod two to three feet apart upon the screen. The one lamp was now so reduced in intensity (by a slide resistance) that its shadow was nearly or quite invisible when the other, stronger lamp was switched on. The stronger (second) light was conveniently turned on and off by means of a knife-switch. The demonstration begins with the weaker shadow seen upon the screen. When the switch is closed and the stronger light turned on[2] this shadow instantly jumps to the shadow (at the right or left) now cast by the stronger light. Upon opening the switch, the shadow jumps back. Several repetitions improve the perception or, as Koffka would say, they 'better the *Gestalt*.' The to-and-fro movements across the screen from one shadow to the other are very striking and unmistakable. Since the shadows are several feet in length, no difficulty of observation is experienced in any part of a large lecture-room.

The demonstrational advantages of such an arrangement may be briefly set down: (1) The entire arrangement can be set up in fifteen minutes out of stock materials in the laboratory. (2) Nothing moves, in the physical sense, to distract the observer, neither rotating discs, shutters nor sliding frames. This simplification is important. The audience sees the essential elements of the set-up, sees no moving parts; and yet the phenomenal movement is unmistakably present. (3) Increase and decrease of the size of the field of movement are made with ease. By moving the upright rod nearer to the light-source, the spatial separation of the shadows is increased, thus

[1] *Amer. J. Psychol.*, 1926, **37**, 77–78, 90–93.

[2] Wertheimer made good use of sliding frames for the production of stroboscopic movements, and Carmichael has more recently described a clever demonstrational device made of a lighted box with sliding frames which bear opaque objects seen by the observer in silhouette upon a translucent glass screen. (See L. Carmichael, A device for the demonstration of apparent movement, *Amer. J. Psychol.*, 1925, **36**, 446–448.)

extending the range of phenomenal movement; by moving the rod slowly toward the background, on the other hand, the total excursion is lessened. (4) Again, by moving either light toward, or away from, the rod, the relative intensities of the shadows are changed. Since much has been said about the rôle of intensity in determining the character of the perceived movement, it is useful to have at hand a simple and effective means of demonstrating to a group the extent of such a dependence. (5) The size and length of the stimulus-objects are readily modified. It has been asserted that a change of size (or mass) is necessary where a relative increase or decrease in the time-interval or in the spatial separation of the objects has been made. We can quickly substitute rods of unlike width or place together two rods of unlike dimensions in the same field to demonstrate any such dependence of movement upon size. (6) By adjusting the length of one of the rods, movement in a vertical, as well as in a transverse, direction can readily be shown. Where the one shadow is three feet long and the other six, the shadows rock back and forth across the field, alternately elongating and contracting. Finally, (7) the demonstration clearly shows how easily the time-interval may be dispensed with in the stroboscopic perception of movement.